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1961

PUBLICATIONS

OF

The American Philosophical Society

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THE MAGIC SQUARE[1]

ROY F. NICHOLS

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DURING the half-century from 1720 to 1770, within the compass of a city two miles square and in a section of that metropolis which can be measured merely in rods, there was some sort of unusual creative force stirring. Within this so-called magic square were established a library, a college, a medical school, a hospital, social welfare agencies, a museum, several schools, a variety of newspapers and magazines, and a learned organization modeled on the British Royal Society. Within this restricted area certain unusual men advanced scientific knowledge, men such as Logan, Franklin, Bartram, Smith, Morgan, and Rittenhouse. Here some three score or more ingenious and responsible men of affairs, experimented, wrote, and talked. They made science their great interest and contributed to its advancement in a fashion worthy of the Age of Enlightenment in which they lived. Why was it that in a little colonial outpost three thousand miles away from the centers of civilization, a few hundred people, living in such obvious isolation, could do so much that was sophisticated for science in so short a time?

The task of explaining why so much of scientific significance happened in such a short time in Colonial Philadelphia is no simple one. Communities like individuals behave, but it is not always easy to explain why. This City of Brotherly Love is a reticent community and has always seemed unconcerned about recording what has been done within its boundaries. In fact one of its characteristics seems to be a sublime indifference to any form of self-revelation. This attitude, if it really exists, is not very helpful to the historian.[2] Whatever answer there is to this question cannot be simple and it must take account of factors existent in Philadelphia from its beginnings.

The colonial project conceived by William Penn was undertaken late in the years of colonial venturing. His plans for an American community were next to the last made for England's Atlantic coast plantations. Such a late-coming stream of immigrants found this to their advantage. The way had been made by others and Penn and his associates could profit by the labors, the hardships, the successes, and failures of their predecessors. Behind them were seventy-five years of experience in planting colonies in America. Two, and in some instances three, generations had already performed their labors. By the 1680's a good deal was known of the art of wilderness conquest. When Penn arrived, Pennsylvania, at least along the Delaware, was not virgin wilderness. He found Swedish, Dutch, and English settlers within his domain and some settlements more than forty years old. Within the eventual bounds of his dream city of Philadelphia there were already some hundred settlers.

Penn was inspired, as a promoter, in various ways which were to be of great advantage to his project and incidentally to the cause of science. He learned how to deal with the aboriginal inhabitants and by making treaties with them won their friendship. Thus Pennsylvania avoided the period of Indian warfare that endangered the Southern and New England colonies. Penn was also something of a businessman. He was coming to a region where already neighbors and supplies were available in New Jersey, Maryland, and New York. He sponsored a Free Society of Traders designed to keep the arteries of commerce open and supplies of food and money flowing. There was no starving time in Pennsylvania. Furthermore, the climate was reasonably equable. His colony did not have to suffer tropical heat and diseases, or New England winters and epidemics.

The late arrival of settlers with the way in a sense prepared, together with the absence of dangers from attack, starvation, and disease, meant that Philadelphia minds and interests were not imprisoned and tied down by the all-absorbing necessities of keeping alert and alive in the

[1] This address was delivered in the Library of the American Philosophical Society before the National Academy of Sciences on November 14, 1960.

[2] I have been much aided by Brooke Hindle, *The pursuit of science in revolutionary America, 1735-1789* (Chapel Hill, 1956) and by Whitfield, J. Bell, Jr., Science and humanity in Philadelphia, 1775-1790, unpublished dissertation in history, University of Pennsylvania.

face of obvious lurking perils. The best minds and many others were free to wander in quest of the answers to questions less immediate and more subtle. Those listening for Indian war-whoops, gripped by the gnawing pangs of hunger, or disturbed by the inner uncertainties of dysentery do not usually make notable contributions to astronomy or higher mathematics.

A second series of circumstances favorable to science arose from the nature of William Penn and his enterprise. The founder of this city and commonwealth believed in design; he was, in modern parlance, a model builder. He would have been at home with the city planners, and he had some vague concept of regional science. He wanted a practical democratic government in which all groups in the community would share. He had ideals of law and conduct which would design Pennsylvania as a model commonwealth.

His surveyor, Thomas Holme, was Philadelphia's first scientist in point of time, a man almost forgotten. He laid the city out in parallel streets at right angles, making each of the resulting squares large enough, according to Penn's direction, so that each man might build his house in the middle of his plat, with ground on each side for gardens and orchards. Five squares, resembling the spots on dice, were set aside for public parks. The whole had a symmetry which has been preserved unto this day although no houses are any longer situate in the midst of orchards. Much of the construction was of brick and it became known as the Red City, a red city of right angles.

Penn not only projected this city plan and an enlightened Charter of Liberties, but he had conceived a spiritual design for his project. His own Philadelphia named to be a symbol of community concord, the City of Brotherly Love, he prayed might be kept free from evil and faithful "in the life of righteousness" to which he dedicated it. For in creating this settlement he designed it as he expressed it, to "serve God's truth and people in that an example might be set up to the nations in the new World for such an holy experiment for which there was no room in the old."

Few community designs have been more enlightened in purpose or exalted in conception. Regardless of what may have happened in Philadelphia in the nineteenth and early twentieth centuries, it is not too fanciful to believe that this inspired design of Penn had something to do with the achievements in his city in the eighteenth century. There is a suggestive power about design which commands attention and shapes behavior. It could not fail to be a determinant in an Age of Reason dominated by concepts of natural law and mechanical precision. What mathematician either terrestrial or celestial does not bow down and worship order? Philadelphia was dedicated to spatial, political, spiritual, and hence to demographic order.

A third factor shaping Philadelphia's scientific interest was one of Pennsylvania's unique experiences among the colonies. Those who shaped its institutional patterns, particularly its laws, were commercial rather than agricultural in their preoccupation. Their business enterprise meant that they did not set up a system of probate law primarily devoted to the English aristocratic policy of keeping landed estates intact and protecting entails. These businessmen were interested in collecting debts and accumulating cash. The laws relating to inheritance and the collection of debts protected merchants and lenders to a greater extent than in other colonies. These circumstances encouraged the amassing of personal property in money and securities.[3] This commercial preoccupation meant that there would be free funds for indulging hobbies, for patronizing ingenious and interesting people and for supporting institutions devoted to providing facilities for satisfying public interest. It meant likewise that there would be more of a world-wide outlook, less of the narrow interest sometimes characteristic of those who spend their lives merely tilling their own soil.

Another significant characteristic was the variety of the population. Philadelphia was a polyglot city. It was a busy port of commerce where ships dropped anchor on arrival from many parts of the world. There was a variety of nationalities. The Quakers, the Pennsylvania Germans, the Welsh, the Irish, the Dutch, the Swedes, the French Huguenots, and the Scots were all represented. This variety of tongue and interest meant that there was much to awaken curiosity and wonder in men, to puzzle them and make them strive to understand what at first was unintelligible. Human relations in daily interchange could not be taken easily for granted but must require a certain effort, sharpening the brains and the powers of observation of even the most humble.

Furthermore, in Philadelphia, unlike New

[3] I am indebted to Alfred W. Putnam, Esq., for this information.

England, there was no ecclesiastical dominance, no hierarchy of any particular cult. The Quakers who at first were most influential in the government were tolerant, their direction came from within, from the "Inner Light," they sought no control over men's minds and only a minimum over their actions. Their religion was not dogmatic, ritualistic, or dependent upon any formal creed. They were not as concerned over the possibility of punishment in the after life, as were those who were more aware of predestination; they were more concerned with making the most of the opportunities for good presented in this plane of existence. On the other hand the Puritan with his tendency to think of fulfillment in the next world found it more difficult to summon either that interest in or curiosity about this one which the true scientist might demand.

This absence of religious dominance was not matched by an absence of religious interest. There was a religio-political rivalry abroad which augured well for science. By mid-century, Penn's heirs had become Anglicans and their governors were at times at odds with the local leaders. To this was added the spice of Scottish controversy. In Britain the Anglican universities, Cambridge and Oxford, standing on the classical educational tradition, were frequently in dispute with the University of Edinburgh and the English dissenting academies which were more sympathetic to the study of science. As Cambridge and Oxford upon occasion controlled avenues of preferment their dominance could be discouraging to those of dissenting connection and a number of them, particularly some with medical and other scientific training, came to America to better themselves. They contributed notably to American scientific interest, including that of Philadelphia.

But at this juncture it is essential to point out that in Philadelphia there was little that was anti-religious, certainly not among scientists. There was as great an interest in religion as there was dissatisfaction with orthodoxy. These students of Newton, those who followed the stars as they moved in their courses, those who were studying the earth and its increase, often became highly religious. They saw in all around them the working of natural law and their thoughts and their emotions moved them to contemplate the nature of the law giver. Him, one of them apostrophized, "I thank Thee that I may think Thy thoughts after Thee." The discovery that men were living in a universe of order and calculable revelation satisfied the religious longings and encouraged scientific investigation; the study of science would be an accumulation of evidence of the existence of God. The fact that there was no orthodox hierarchy to say them nay was a great advantage to the new explorers. In Philadelphia there was none to interfere with their efforts to satisfy their curiosity or their freedom to proclaim their results.

Finally in rendering this accounting we must take heed of the spirit of the times, of the Zeitgeist. The seventeenth century had opened the eyes of men to a new vision of human destiny. The discovery and settlement of America and the labors of Isaac Newton had shown Europeans a new world and a new universe and what they were. Man lived in a great cosmos which could be revealed by the study of its mechanics, for it was a world of calculable law. Men sought to describe the wonders of this universe by compiling a "natural history." In it there were many questions and the answers to no small share of them were believed to be discoverable in America.

The circumstances controlling the spirit of the times are many and various, such disparate and diverse situations as the distribution of adrenalin in certain people, the glandular orientation of others. Then there is the delightful polysyllabic "serendipity," the prevalence of the capacity on the part of individuals when seeking for one thing to find another and then put their unexpected discovery to good use. Perhaps, however, it is just as revealing to refer to accidents, the accidents of the existence of certain people at certain times and places, of things happening in the Greek kairos, the fullness of time.

Perhaps it was accident that William Penn should bring to America with him as the eighteenth century approached, a well-educated Irishman, James Logan, as his secretary. This young Quaker proved to be an excellent politician and administrator, a man of wealth-acquiring capacity, a collector of books and an experimental botanist. As was the custom in those days, Quaker merchants carried on extensive agencies through many distant correspondents. In London one such, Peter Collinson, like many another wealthy Englishman, kept a fine garden as a hobby and had become an amateur botanist. Because of this scientific interest he had been made a Fellow of the Royal Society and had become a friend of the great botanist Linnaeus. He was likewise a business and a scientific correspondent of Logan. The latter sent Collinson papers de-

scribing his work on the sexual behavior of plants and the merchant had secured their reading before the Society and their publication in its *Transactions*. Linnaeus recognized Logan's work by naming an order of plants after him. Logan in the meantime had been aiding science in another fashion. He had befriended a young and ambitious farmer boy from Darby, John Bartram. This lad had become interested in flowers partly because he admired their beauty and symmetry and partly because he was of a practical turn of mind and interested in their medicinal properties. At any rate, when he bought himself a farm on the Schuylkill, he undertook not only to make a living on it but to plant an unusual garden where he would assemble the American flora. That garden is still growing. Logan heard of him and encouraged him by loaning him books and helping him master taxonomy. Meanwhile Bartram, unschooled though he was, sought to learn enough Latin to enable him better to classify.

Collinson had been accustomed to secure seeds from all over the world through his mercantile correspondents, but time had shown him that they were not the best seedsmen and their consignments often did not measure up to his hopes and specifications. His problem of American imports was particularly difficult. At length Joseph Brientnall, another Quaker merchant in Philadelphia who knew Logan and something of botany, suggested to Collinson that Bartram was the man to collect and ship the great variety of seeds he wanted. Collinson found in this advice the answer to his needs. Bartram proved extremely helpful to him and on his part undertook to promote Bartram's reputation and business in England. Bartram in the end likewise presented papers to the Royal Society though not on botanical subjects. He became the King's Botanist and traveled widely in the colonies seeking new varieties. He published some journals, but would never follow Collinson's urging to write a formal botanical work. Thus a happy though somewhat accidental conjunction of persons, Logan, Collinson, Brientnall, and Bartram did much to make Philadelphia a center of interest in "Natural History."

A second accident was the arrival on an October Sunday morning in 1723 of a seventeen-year-old Boston lad. He was a printer who young as he was had already had some difficulty in his home city because of advanced views on politics and religion. He had concluded it was wiser to seek his fortune elsewhere. He tried it first in New York City, but William Bradford, the printer there, had no opening and sent him on to his son in Philadelphia. So in Penn's city Benjamin Franklin made his home. As he records it, he "began to have some acquaintance among the young people of the town that were lovers of reading with whom I spent my evenings very agreeably." By 1727 these book-reading youth had formed a secret club which they called the Junto. Within four years they felt that their books should be pooled and made available to others in the city who desired to read. To that end they established the Library Company of Philadelphia which eventually built a structure on Society Hill to house their treasure. The American Philosophical Society has recently recreated this building on its original site. Joseph Brientnall, noticed above, was also a member of the Junto and Secretary of the Library Company. He and Franklin sought Collinson's aid in the purchase of books for the Library, and Collinson and Logan helped them, the former adding an occasional gift of his own. Through Collinson's intercession they granted Bartram the use of the Library Company's volumes.

Franklin's cultural enterprise did not stop with setting up his print shop and with organizing a club and a library. He quickly founded a newspaper and got into politics. He made his start in this great game by advocating the issuance of paper money and then when his plan was voted, got the contract for printing it, a tactic he had tried out successfully in New Jersey. By 1736 he was clerk of the legislature and thus on the inside of lawmaking, and deputy colonial postmaster-general which gave him control of the mail service. He was in a key communications position. Almost on the side, one would think from his autobiography, he was making money from his print shop and stationery store. He continued vigorous in his promotion of institutions, and was always on their subscription lists and on their boards. A college, a hospital, a fire company, a Masonic lodge, a Presbyterian church, at least for a while, all these found him useful and his imagination was likewise stretching out to include an American edition of the Royal Society.

He was continually contriving inventions and developing theories. He invented a stove and experimented with almost anything from bifocals to electricity, and it was in this latter field that he was to gain his greatest scientific fame. One of the Scottish physicians who came over to America,

Dr. Adam Spencer from Edinburgh, gave some illustrated lectures on electricity which Franklin heard. Later when Peter Collinson sent over a glass tube for electrical experiments as a gift to the Library Company, Franklin himself took up experimenting and wrote up the results in letters which he sent to Collinson and which the latter put into print. For this contribution Franklin was elected to the Royal Society.

During his electrical enthusiasm Franklin's imagination was busy conjuring up more new institutions. He was thinking in educational terms, regretting the absence of any satisfactory academy or college. Franklin and certain associates had been discussing a plan for a school. As usual Franklin wrote and printed a working paper and an academy soon came into being. It had as its rector a Glasgow-trained divine, Rev. Mr. Francis Alison, who had crossed the sea in 1752. Just about then Franklin received a pamphlet outlining a plan for a college sent him by an Aberdeen-trained clergyman who had likewise come to America seeking employment. New York City was also thinking in educational terms and the dominie undertook to do business. Franklin immediately invited Rev. Mr. William Smith to come over to compare notes and New York thereby lost him. Within a year the Academy was the College of Philadelphia. Mr. Smith was its Provost and Mr. Alison its Vice Provost. In a day of classical absorption these two Scots, with Franklin behind them, set up a curriculum, forty per cent of which was devoted to science, and the good doctor was impressing upon his charges his belief that education was a continuous process which needed to be continued throughout the days of their years. The University is just about catching up with its first Provost now.

In this train of accidents there occurred another one of these "accidental" convergences. While Smith and Alison were getting the College under way, the English-Quaker interests were to make another contribution. One of the associates of the London merchant, Collinson, in the patronage of things American, was a Quaker Physician practicing in London, Dr. John Fothergill. He had befriended Franklin and to him many traveling Americans sought introduction. Young men desiring to study medicine were eager to receive his help and teaching.

Mr. Smith and Mr. Alison had brought the first class of the College of Philadelphia to graduation in 1757 and among its members was John Morgan, whose ambition was the study of medicine. As was customary in those days he apprenticed himself to Dr. John Redman, a Dutch-trained Philadelphia physician, who had also learned medicine the hard way as a surgeon in the French and Indian War. But he was not satisfied and went abroad with a letter from Benjamin Franklin to Dr. Fothergill. He studied in London under the Quaker physician and learned at least of the existence of another medical student from Philadelphia, William Shippen, Jr. After a year in England, Morgan, following a well-trodden path, went to the University of Edinburgh to complete his medical training and to receive his M.D., presenting a thesis on pus. It was during this period of study that he was seized with a new ambition. He would work to establish a medical school at his Alma Mater.

In the meantime, Dr. Fothergill had been encouraging his other pupil, Dr. William Shippen, Jr., to embrace the same idea. He advised him on his return to Philadelphia to offer at the hospital a course of anatomical lectures and so that he might illustrate them, gave a series of anatomical drawings to the Pennsylvania Hospital—then located in the woods on the outskirts of the city—in the hope that these lectures might be the beginning of medical education in America. This Shippen did in 1762. Two years later the great medical founder of the Pennsylvania Hospital, Dr. Thomas Bond, another of Franklin's associates, offered a course himself at the Hospital.

Dr. Morgan meanwhile had gone from Edinburgh for further study to Paris and to Italy and then home to Philadelphia. His purpose too had been announced by Dr. Fothergill, who in introducing Shippen had written that he would be followed by "an able assistant, Dr. Morgan," both of them he thought "if suitably countenanced by the legislature, will be able to erect a school of Physic amongst you." In 1765 Morgan arrived but he was not willing to assist anybody. He went directly to his old master, Dr. Smith, and to the trustees of the College. His purpose was persuasive; his proposals were accepted and he was appointed Professor of the Theory and Practise of Physic in the College of Philadelphia. This event he celebrated by a two-day inaugural address. In the course of this speech he rather casually referred to the man whom Fothergill had suggested he assist as a competent teacher who might possibly be made Professor of Anatomy. This Shippen never forgave although he accepted the professorship; the first medical school was founded in an atmosphere of professional rivalry and something

akin to hate. Morgan in establishing the school took advanced ground for he insisted that doctors should be trained scientists as well as practitioners of physic.

The final accident in this series forms another link in the significant chain of personal relationships which created the leadership in Philadelphia's scientific enterprise. Science here has owed much to mechanics and to inventors, to surveyors and to others who were busy with applications of basic principles they sometimes may not have understood. Science has owed much to the technicians as well as to the natural philosophers. These men were likewise "promoting useful knowledge."

One of these mechanics was David Rittenhouse, born on the Wissahickon, son of the operator of a paper mill. Like Bartram and Franklin, he had little or no formal education, like Logan he was enamored of Newton and his *Principia*. He started as a clockmaker who also did surveying, thus joining the ranks of James Alexander, Cadwalader Colden, Mason and Dixon, and those other mathematicians and astronomers who charted the wilderness, plotted its meteorology and brought precision and calculation to a high degree of efficiency. These men contributed much to the local interest in science. Rittenhouse attracted Provost Smith's attention by the construction of an ingenious orrery, a mechanical device illustrating the celestial universe and the movements of the planets. Princeton bought it up under Smith's nose, but Rittenhouse, however, presently built another for the Provost, and eventually Smith conferred upon Rittenhouse the degree of master of arts. In the course of time the latter presented a paper before the American Philosophical Society, and took an active part in making the elaborate plans for the observation of the transit of Venus in 1769. His increasing involvement in the interests of the magic square caused him to move into Philadelphia from his country cot in Norriton and in due course he became a Professor and Vice Provost of the reorganized University of Pennsylvania. The world acknowledged him to be a great astronomer and mathematician.

All of these men here cited as illustrating the accidents of personal relation and leadership were, save Logan, members of either the Royal Society or the American Philosophical Society (in one form or another) or both. This latter body which came to final organization in 1769 represented one of the secrets of Philadelphia's scientific primacy, a vivid group of active minds, some professional, many amateurs, a number enriched with the profits of commercial venturing; men who would meet and talk, organize and contribute of knowledge and substance.

So it was that Philadelphia became the center, the place where things happened, where there were intellectual excitement and curiosity whetted by discoveries. It did not happen in New York, then a somewhat sleepy provincial seaport under a lethargic Dutch influence. It was Philadelphia that was the British commercial center in close touch with all that was brewing in Great Britain. Many of its leaders were eager to catch up with, or at least to reflect British scientific sophistication and it was possessed of an interest and a dynamics which were as yet foreign to New York City. The eighteenth was not the nineteenth century.

In conclusion the answer to the question may be summarized. In eighteenth-century Philadelphia there was the combination of a receptive community and a leadership with the intellectual and the material means required in such a unique epoch. Here were men of many minds and broad interests in a new community in an Age of Reason. Had it been an old community, had it been one with a dominating pattern of interest, had it been one with a set, homogeneous culture, had there been a different set of personalities, had the age been of a different intellectual cast, the scientific achievement would probably not have been forthcoming.

Philadelphia was a city with a notable plan and a noble purpose. Here was a new commercial community, a place where venturing paid dividends. It was a polyglot society where men of all tongues and faiths congregated in a commonwealth. It was a community where things happened, where there was an air of expectancy. Here people were on the lookout for new objects of interest, new knowledge, new answers to questions both old and new. What was even more important there was a leadership which included men not only of means but men with sufficient interest, leisure, and industry to read, investigate, talk, and write. The subjects of their interest were difficult ones, hard to understand; the mind must wrestle with them and often be thrown hard on the floor of noncomprehension. But in this city, professional and amateur, academician and man of affairs, embraced a common cause and labored together, not sporadically or casually, but in organized operation which

they designed and supported. The discovery of new truth and the promotion of useful knowledge were made a community enterprise, the fruits of which were to be shared with the world. It was a very exciting and fruitful concentration and expenditure of energy. Brains, wealth, and government were brought together in enthusiastic support of discovery for the benefit of mankind.

Perhaps I should stop here but I cannot forbear to follow Franklin's example and point a moral. Looking back has little value unless it invites a forward view. Sighing for golden ages is a waste of breath unless the cleansed lungs are filled with a fresh blast that gives energy to race forward.

The twentieth is not the eighteenth century; all we can do is to use the eighteenth century to remind ourselves of new opportunity as Philadelphia is trying to do down here on Society Hill. The opportunity I believe is this. We cannot bring back the relatively simple science of the Age of the Enlightenment when men were entering the gates unlocked by Newton. We are now in an age of relativity and atom smashing, of genetics and molecular analysis. It is not possible probably for a group of apprentices or even successful men of affairs to sit around of an evening discussing the hideous pages of algebraic and other mathematical symbols which are now called science, physical, biological, or social. In fact there is, I believe, danger that men of great understanding are so concealing themselves behind the locked doors of jargon, symbol and quantified concept that what they know is in danger of being lost to common interest. The community knows that something is going on, that magicians are working in classified dungeons, upon secrets which may destroy us.

But nobody knows better than you do that scientists should not be so described. There should be I believe an even greater hope of salvation than fear of destruction to be anticipated from their efforts. The possibility of such salvation should be told to the society that pays the research bills. The wealthy amateur can no longer be depended upon as he could in the eighteenth century to give adequate support to advancing discovery and spreading enlightenment. We must now depend upon corporate enterprise and society, i.e., government, for this support. The more business, government, and the public know and understand of the objectives and achievements of science the more we may be confident that they will be willing to accord that support. But their interest now is centered on certain types of scientific application which seem immediately useful. Appliances or practices which increase efficiency and cure the ills of mankind are attractive. But where is the support coming from for basic research, who will support research involving the nature and properties of the square root of minus one? These Philadelphians in the eighteenth century were engaged in discussing electricity, gravity, the transit of Venus, and other questions in basic science which perhaps did not have in their minds too much immediate application but they wanted to satisfy their curiosity.

Scientists today must be more anxious to excite the imaginations, the curiosity, and the hope of a somewhat frightened society. The times demand an effort to show to the community the values which may be discovered, conserved, and insured by adequately supported programs in basic science. Our fellow men by and large are dazzled by the extent and complexity of our knowledge, they have no comprehension of the extent of what we do not know. Some means should be taken even if it involves resorting to Madison Avenue to open the eyes of the public to the needs of those interested in basic research. It is particularly necessary to convert our own students to the idea that this should be their concern in choosing careers. But how can this be done without support? It would appear that in the eighteenth century, in Philadelphia at least, basic science was something of a community project eliciting an appreciable amount of general interest. Can this state be recreated on a national scale?

In 1776 a group of men since become famous met across the street and around the corner from this hall. They faced an emergency and made a world-noticed declaration. Mindful of their example, would that the nation's scientists might here or elsewhere highly resolve to restore science to the community. They might thereby be instruments in saving the independence once so bravely here declared.

EXCELLENCE AND MEDIOCRITY IN A UNIVERSITY

JOSEPH H. WILLITS

Formerly Director, The Educational Survey, University of Pennsylvania

(*Read November 10, 1960*)

THE University of Pennsylvania has been conducting an experiment called "The Educational Survey." I was privileged to participate in that experiment and now welcome the chance to share a little of it with the members of this Society. I say a little because the studies of the Survey and their summaries and appendixes amounted to a four-foot shelf of books. Most of the rich material contained in the twenty-eight major studies made of the different schools and problems of the University, must, for considerations of time, be omitted. Only a few studies will be discussed.

HISTORY, METHOD AND ACKNOWLEDGMENT

The idea of the Survey grew out of unrest on the campus—unrest in the faculty and unrest in the Board of Trustees, and the questing of a new president. The Trustees had appointed a Survey Committee of their own—under former Justice Owen Roberts—to consider educational policy, administrative and fiscal policies, and athletic policy. The faculty—for the first time in the University's history of over two hundred years—had formed a University Senate to provide the faculty with a university-wide town meeting and to make possible a larger voice in the University policy. Dr. Gaylord Harnwell, a member of the faculty, had been chosen president after the resignation of President Stassen on June 30, 1953. He was completely dedicated to increasing the excellence of the institution. Trustees, faculty, and administration joined in the decision that a thoroughgoing study of the University should be made.

There is an impertinence involved in accepting an invitation to study a whole university. But I cared deeply about the University of Pennsylvania and owed it much. I had long felt that some university should make a really intensive and objective study of its strengths and weaknesses as a whole. Further, it was clear that every university and college in the land was under a perpetual obligation to greater excellence. No matter how good they were or might become, they would not be good enough. For the foreseeable future, our problems will be growing faster than our capacity to comprehend and deal with them.

You may receive a biased report on this Educational Survey from me. All the mistakes we made—such of our galleons as limped a little lamely to port and all the opportunities we missed—do not alter my confidence in the basic values of the Survey. But you have some protection from my biases; in this audience sit members of the University administration, faculty, and trustees; also professionals from outside who directed studies; and my colleagues in the central staff of the Survey.

For you to understand this experiment at Pennsylvania, it is necessary that I discuss some prosaic questions of history, method and acknowledgment. Some basic decisions of policy were made at the start or evolved in the process of making the Survey:

1. We believed that it would be an exercise in futility as well as an impertinence for two or three persons in the central survey office to try to study everything from law to microbiology and from chemistry to the classics.

2. In most cases professional competence and objectivity would be better served if the persons who made the initial basic studies were the best available professionals from outside the institution. In consequence of this policy one hundred and twenty-six persons from outside participated in studies. Only a minority of these came for major studies and remained for a semester or an academic year. (This policy of depending on outsiders did not obtain in every instance. For some studies insiders were better equipped than outsiders—and were used usually with great success.)

3. But dependence on outsiders—invaluable though their contributions were—was not enough. Extensive participation by numbers of the Pennsylvania faculty and administration was equally necessary because of their more intimate knowledge and because they carried the responsibility for the future.

This participation was brought about in three ways: In the study of most schools a faculty committee was asked to state—for the benefit of the outside consultant—what they felt to be the most important problems for the consultant to consider in his study.

While the outside consultant was making his study of a school or a problem a Review and Appraisal Committee composed of members of the faculty and administration was appointed to serve as adviser to the consultant to use as he wished. After the consultant's study was completed, this committee reviewed it, appraised the study, supplementing or controverting it as seemed warranted. But it could not amend the report; that stood as the author had written it.

A third agency of faculty participation was the Faculty Advisory Committee to the Educational Survey. This Committee—appointed by the President after consultation with the Provost and with the President of the University Senate—consisted of thirty-two members of the faculty representing the University as a whole. Before the officers submitted any study to the President it was submitted to this committee for criticism, dissent, endorsement or supplementation as they saw fit. Any dissents were noted in the officer's submission to the President.

These three steps meant that about three hundred members of the faculty and administration were formally—and most of them actively—participants in some study or studies of the Survey. Beyond this number were many, many more who studied and worked independently on the reports of the survey in order to support, controvert, supplement, or just to understand them.

In short, this study of Pennsylvania was not one in which a few persons sat down and, in their lonely wisdom—after a few year's work—submitted a report about the entire University, that report to be considered or ignored as circumstances might determine. Dr. Glen Morrow, Professor of Philosophy and formerly Dean of the College and Chairman of the Faculty Advisory Committee on the Educational Survey, spoke of faculty participation in these words: "I am impressed with the magnitude of the cooperation that the Survey has elicited from the members of the faculty." But another step taken by the President was to carry this process of active faculty participation further.

After a study had been completed by the Survey, and subjected to the above processes and submitted to the President, it was subjected to further processes of faculty and administrative scrutiny. The President has described these processes in his Annual Report for 1959 in the following words:

> At this point, the Survey's work on individual studies was completed, and the administration has then carried the matter forward under a procedure which insures the widest consideration of all those having a relevant interest. This includes: (*a*) Referral of reports to the Educational Policy Committee of the faculty and referral by it to the Educational Council or the Senate if there are major policy questions to be resolved. (*b*) Referral through the Provost of reports to the parts of the University to which they apply, requesting a statement of attitude and plans for implementation. (*c*) Review of accumulated materials by the President and transmittal of his recommendation to the appropriate sub-division for implementation of decisions not previously acted upon. (Through local participation in the study by members of the pertinent faculty, the resulting recommendations were, in many cases, actually put into effect in their original or modified form prior to the completion of this procedure.) (*d*) Referral to the Trustees of final Survey reports and a digest of all materials relating to studies on which action by them is required.[1]

You can see—from the above—that Survey studies were not neglected, that they were not just a row of books on a library shelf at the end and that, in most cases, they penetrated the blood stream of the University. In short the Educational Survey was a *process* as well as some twenty-eight studies. The years of the Survey were years of vigorous ferment, action and reaction, as well as of study. Many recommendations—or *a preferred alternative*—were adopted, as President Harnwell has above suggested, long before the final report of the Survey was completed. President Harnwell's 1959 report, "Assaying a University," should be read for elaboration of this theme.

It should be noted that no invasion of academic freedom was involved in this process because the Survey had neither legislative nor executive authority. Decision-making remained as it had been before in the hands of the usual decision-making bodies—the faculty, the administration, and the Board of Trustees.

It should be repeated that three basic policy

[1] To complete the statement of Survey studies it should be noted that Julia M. H. Carson, Assistant Director and Editor of the Survey prepared a three-volume summary of the twenty-eight Survey studies and the Director of the Survey prepared a two-volume commentary on university policy as suggested to him by the Survey.

objectives were fairly successfully achieved: preparation of most of the basic studies by outsiders; active participation by faculty and administration; a process of ferment and action during most of the six years of the life of the Survey. You can see from what I have said how immense is our debt to many persons. Nearly all the good in what I say to you has come from others. I assume responsibility for the errors and shortcomings.

We are very deeply indebted to the great scholars and scientists from outside who set aside their own professional work to give of their experience and professional wisdom and prestige to a study of Pennsylvania's problems and policies. My personal debt to these professionals is beyond words. Likewise, my debt to the faculty cannot be exaggerated. As Glen Morrow pointed out in the address already quoted: "For make no mistake about it. This was not a congenial task; in fact it was highly disagreeable. No one likes to be surveyed . . . or having the defects of himself and his colleagues spread before the academic community, especially when some of the defects were the product of misinterpretation. . . ." To the faculties of the University—who often had had to make bricks with insufficient straw and had not always been well tended by top administration in the last fifty years—but who cooperated with the Survey, I wish to record my deepest gratitude. This prevailing attitude was an expression of honesty, courage, and caring. There were a few exceptions, of course; but that was to be expected.

I would be remiss if I did not also express my thanks to the President of the University for offering us the opportunity, for giving us freedom, for advice when it was asked, and for seeing that each study received the most careful consideration. These thanks apply equally to the three Provosts who held office during the Survey—Dr. Edwin B. Williams, Professor of Romance Languages; Dr. Jonathan Rhoads, Professor of Surgery; and Dr. Loren Eiseley, Professor of Anthropology.

My warmest thanks of all go to my colleagues in the central staff of the Survey, to Dr. Malcolm Preston, Professor of Psychology and successively Assistant Director and Co-Director of the Survey, Julia M. H. Carson, Assistant Director and Editor of the Survey, Mrs. Charlotte Dodds, Executive Secretary and Administrative Assistant to the Director, and to Dr. Marion Elderton, Research Associate and Administrative Assistant to the Director.

SOME FINDINGS OF THE STUDIES

I shall now pass from these dull introductory questions of history, method, and acknowledgment to the findings of the studies. What happened from this massive release of creative energy? Here I must skip lightly since twenty-eight major studies cannot even be adequately summarized in twenty minutes—let alone discussed in their details and broader implications.

I stress, first, the many greatnesses in the University of Pennsylvania. Studies that seek for ways to greater strength must be critical. In consequence, the emphases may seem to be on the pathological side. To offset that, I wish to say at the outset and with the greatest emphasis that one of the delights of this Survey has been to discover so many small and large sectors, and so many individuals on the faculty, administration, and Board of Trustees that stand comparison with the best anywhere. These persons and sectors are worthy descendants of the great figures who, in more than two centuries, have built the Pennsylvania of today. To persons of such quality an educational survey may be superfluous. But we keep always in mind that the central object of the Survey is to see more individuals in the image of such persons.

THE SPIRIT OF PENNSYLVANIA

The most important value in any university is the spirit of the institution. It is a subtle complex and a distillation from many forces. Ibsen has said "The spirit of truth and the spirit of freedom: these are the pillars of society." In a university these are basic but even they are not enough.

Provost Eiseley has said that it is the business of a university teacher—any teacher—to create the future in the minds of men. He continues, quoting Whitehead: "The sense of greatness is the ground work of morals. We are at the threshold of a democratic age, and it remains to be determined whether the equality of men is to be realized on a high level or on a low level." That is the central concern of a university. A university that is neutral or negative in these respects will send out men content with the mediocre in mind, morals, and taste.

In every university, there is a constant tug between the forces that strengthen ideals and place them first and those that erode ideals and subordinate them to the personal ends of power and selfish advantage. Pennsylvania is no exception. She has great traditions. The sense of freedom is in

her warp and woof. The members of her faculty and family as a whole exhibit that loyalty which Pennsylvania so conspicuously evokes. Her integrity rating is high. Moreover, Pennsylvania is modest; she has rarely tooted her own horn. And the presence of many great men and women in the university family creates an atmosphere of greatness of spirit.

However, over the last fifty years, circumstances—as in most universities—have here and there produced erosion of spirit. A part of this was discouragement that grew from inadequate finances. A part grew from frustration resulting from Pennsylvania's occasionally not being well tended at the top. Pruning was inadequately done in places. A part was a consequence of the great tradition of freedom coupled with inadequate salaries so that, in cases, professional self-seeking was permitted to blur the vision of the highest university goals. An evidence of erosion was an occasional parochialism that registered little interest or joy in the achievement and problems of others in the university family.

The sad fact about erosion of spirit is that it yields but slowly to change. Happily in today's Pennsylvania these compromises with true excellence are no longer necessary. To insist throughout upon the standards of a great university without diminishing the freedom essential to creativity is one of the subtle tasks of the institution's leadership.

CLARITY OF PURPOSE AND ADHERENCE TO ITS PURPOSES

What kind of a university should Pennsylvania strive to be?

The twenty-eight studies produced a wealth of suggestions on this point.

A supplement to these suggestions was sought by consulting leading members of the university family. I addressed a letter to two hundred and fifty-five members of the faculty, the administration, the trustees, the alumni and other friends of Pennsylvania. President Harnwell sent the same letter to fifty-nine thoughtful citizens most of them from the Philadelphia metropolitan area. One paragraph from that letter will give you its gist:

> But I am not primarily seeking answers to these particular questions. I am asking you whether you will state your sense of the function of Pennsylvania in today's and tomorrow's world in the wide terms in which Cardinal Newman a century ago set out his "Idea of a University." What do we wish to create? What is it uniquely within our power to create? In teaching? In discovery? In professional education? In the relationship of each to the others? In community service? And what are our communities?

Replies were received from: 68 members of the faculty, 39 trustees, 57 associate trustees, 91 officers of alumni organizations, 59 prominent citizens.

The purpose of the letter sent to these persons was chiefly educational. But very few of the replies received called for their being swept under the rug. Most of them were thoughtful attempts to wrestle with the problems of purpose. There were differences in this or that but there was an unusual emphasis on certain points that were basic.

Excellence and high standards were earnestly desired. The authors of these letters wanted these:

> Pennsylvania the best rather than the largest.
> Aim high in academic standards.

The creation, over the years, of six schools in the health field rather than concentration of resources on a few—especially the School of Medicine—has seemed to me to represent a dispersion of human and financial resources. Likewise, five different athletic policies in the last fifty years seem to indicate lack of a clear sense of direction, which the authors of letters felt that Pennsylvania had lacked during the last half-century. They commented as follows:

> She has not been sufficiently selective in her growth.
> Pennsylvania never turns anything down.
> Pennsylvania moves in too many directions at once
> No university has the financial resources to offer courses in everything.
> The University is spread too thin.

The authors of these letters further felt that Pennsylvania should concentrate on her strengths.

> It is better to be best in something than mediocre in all.
> A university is distinguished by what it does superbly—and by what it refuses to do.
> Pennsylvania should be more selective in her admission of students and in her appointments to the faculty.

One faculty member stated his views in the following cogent words:

> Pennsylvania is at an important cross-roads in her growth—Pennsylvania is reputed to offer fine higher education in the great university tradition to undergraduates, graduates and professional students. This reputation is the University's greatest endowment. However, it is an endowment that stands in constant

need of protection and enhancement. It is now only partly merited, and unless it is deserved it will be lost. The University's primary function is education of young people capable of and willing to devote themselves to full-time and mature study of the liberal arts, the sciences, business and the professions. For them the University should preserve, transmit and develop sound cultural and wide-visioned professional education. The University cannot afford any function that threatens to detract from this primary mission.

This author spoke further in the following terms:

> Danger is to be feared from demands both of the metropolitan center in which the University of Pennsylvania is located and of the technological world at large. As a well-thought-of large school, the University will constantly be tempted to dissipate its energies and resources in (1) part-time instruction for underprivileged scholars who cannot afford full-time student careers, (2) cultural edification of the adult community, (3) narrow vocational training, (4) summer school for students who have failed to keep up during the long session, (5) contract research in narrow technologies that promise no advancement of science. Fortunately the community and the country do not need the University of Pennsylvania for accomplishment of these valuable but inappropriate ends. Other local institutions are delighted to offer part-time college and adult education and are eager to give narrow vocational training. The United States is full of institutions competing for virtually all kinds of contract research. The University of Pennsylvania can withdraw from the field with a clear conscience, and without imperiling its public relations. If these activities are continued and developed at Pennsylvania they are likely to absorb faculty time and interest, attract faculty recruits who tend to prefer peripheral activities to deep searching study and sound instruction, or hold out financial attractions that the underpaid teaching profession find it hard to resist.

If Dr. Abraham Flexner had been directing this study of Pennsylvania, he would not have needed all of this Puritan advice to use the surgeon's knife with the greatest vigor and excise large sectors of the University's activities. Lord Lothian, when (as Philip Kerr) he was Secretary of the Rhodes Trustees, reported that Oxford would have taken a reverse attitude toward the speed of change.

This Educational Survey has followed neither extreme. It has been primarily concerned with the policies and choices the University follows in the future. We hope that there will be more critical selectivity for the future. So far as the lines of present activity are concerned, we believe the studies and commentaries have made clear what activities are central and what activities are peripheral. The judgment of what should be continued and what should be excised is an administrative one. It should be based on the value of the function, the excellence with which it is being performed, the cost of the enterprise, and the supply of funds available for support, the persons dependent on the activity concerned who are under permanent appointment and the public relation factors involved. It was my judgment that a list of black balls drawn up by the Survey would do more harm than good. The facts and analyses point the policies; it is up to the faculty, administration and Board to apply them.

President Harnwell has taken steps to carry out this program. A joint Committee of Board, administration, and faculty has been appointed to make long-run plans for the institution.

In general, Pennsylvania will and should remain a multi-purpose institution. It will and should continue to be: a center for research and scholarly inquiry; a center of graduate education in the arts and science; a center of professional education; a center for undergraduate education; a servant of certain publics.

It is my opinion that Pennsylvania must discover and apply the severe limits to her activities by hard choices *within* these five lines of activity. In general, there will and should be a gradual shift in emphasis to more advanced work. And always she should aim at greater excellence in what she chooses to undertake.

THE FACULTY—THE BASIS OF EXCELLENCE

Before this audience it is not necessary to labor the point that no great university was ever built on a weak faculty.

Nor is it necessary to repeat how blessed Pennsylvania is with talent of the highest order at so many parts of the campus. One of the delights of the "returnee" to the Pennsylvania campus is to discover so many persons with that power of mind, the dedication to teaching and scholarship, that integrity and that generosity and grace of spirit—in short, all those qualities which evoke similar qualities in students and colleagues. Such men bring distinction to any university. Yale's Sumner, Wolsey, Crittenden, and Gibbs; Harvard's Shaler, Langdell and Lowes; the National Bureau's and Columbia's Wesley Mitchell; Columbia's Dewey; Chicago's Michaelson, Manley, and W. J. Thomas; Wisconsin's McCarthy; Johns Hopkins' Welch and Osler; Cornell's White; Cambridge's Rutherford and Alfred Marshall;

yes, and some which were on no faculty such as Gibbon and Darwin—a university seeking the highest excellence should desire to have its faculty studded with such persons as these. But such names do not just happen. They have to be discovered and be given a chance to grow and germinate. Nothing is more pathetic and futile than a university which seeks to decorate itself by buying great names. Baseball clubs have learned the futility of that policy. A great faculty is the result of taking pains at many points, of having many people in strategic positions who, as Dr. David Goddard has said, "know what quality is and won't settle for less," who can choose able youngsters and know how to serve their growth and who have some inducements to offer as well as a climate that attracts and holds.

The studies of the Survey revealed that at Pennsylvania—as at most universities—there is the widest variation in faculty strength from department to department. This arises in part from differences in funds available but it is influenced, also, by many other factors.

Now that the effective leadership of President Harnwell is producing a large increase in funds for faculty salaries and aid, it seemed in order that Pennsylvania should take every possible step to insure that, in the future, every post on the faculty or instructional and research staff should be filled with the best persons available for the opportunity, and whom the climate can attract and hold. Therefore, the first study initiated by the Survey was entitled "Towards an Affirmative Policy on Faculty Personnel." The responsibility for the study and report on this subject was placed on an able committee of insiders of which Dr. David R. Goddard,[2] then Chairman of Botany, was chairman. Since the members of this committee were all busy men, Dr. Richard H. Shryock, then Director of the Institute of the History of Medicine of Johns Hopkins University was asked to assist the Committee by preparing a study on faculty personnel at Pennsylvania. There were, therefore, two reports on this subject. The Shryock study and report discussed the wider university setting in which faculty excellence can flourish as well as many specific problems of Pennsylvania. The Goddard report was a highly specific code of policy and practice for Pennsylvania.

Dr. Shryock discussed such matters as the following: the differences between higher education in this country and in Europe and Great Britain; the degree to which basic liberal education is achieved in the schools in Britain and in Europe; and the degree to which the failure to do so here forces universities to teach much elementary work to their own disadvantage and inner conflict; the tripartite government of universities; the dignity and status of the faculty; the dangers of excessive inbreeding; the stake of the faculty in the selection of administrators.

Dr. Shryock's study was published by the University Press. For some reasons for which I am to blame but which is now quite incomprehensible to me, Dr. Goddard's committee's study was not published with the Shryock study. Perhaps it was not published because it was a highly specific code of policy and practice for Pennsylvania faculty, administration and Board of Trustees. Although pointed at Pennsylvania, it had relevance generally for it dealt with such problems as the following:

The type of person desired on the faculty: A good teacher with scholarly drive; High ability but a diversity of background and philosophic outlook.

Adequate procedures in the recruitment of new talent;

The specific responsibilities in insuring faculty excellence from department and department head up through Dean and Faculty Personnel Committee to Provost and President;

The responsibility for "weeding out";

Tenure—a considered decision—not a subject of drift;

Salary Scales: Comparable to Yale, Columbia and Cornell; Flexibility to recognize merit;

Position of the Faculty: Not comparable to employees of a business corporation;

The greatest essential of all: To know what quality is and not settle for less.

It is unfair to quote only a few items from these two admirable studies. They should be quoted in full or not at all. They constitute the contribution of the Educational Survey to a future with only the minimum of missed opportunities in recruitment, appointment, promotion and elimination. This is the most important subject dealt with by the Educational Survey.[3]

[2] Now Chairman of the Division of Biology.

[3] There should follow here a section on students as human beings in the University Community. But students as students are dealt with at many points in this statement. For considerations of space, this section on students as human beings is omitted.

EDUCATION AND RESEARCH

A. *Pennsylvania as a Center for Research and Scholarly Inquiry*

At the core of any university lies the effort by faculty and students to expand the horizons of truth and knowledge. In his study of the Pennsylvania School of Education, Paul Woodring says of research: "This is the cutting edge of the University." The central position of this effort to push back the boundaries of ignorance is what distinguishes a great university from a mediocre one. Research and scholarly inquiry are the spring from which university teaching continually renews itself. If shared with students, research and scholarly inquiry can convey the methods, the spirit and the zest of scholarship and science. The immense growth of research in business does not foreshadow a relative decline of universities as research centers and an increasing preoccupation with purely pedagogical functions. The reverse is true. If universities recruit great persons and maintain high standards, they have no cause for concern. They provide the best opportunity for free inquiry.

In any fundamental sense teaching and research are not competitive, but are mutually dependent, if the spirit of men be right. Research and scholarly inquiry without teaching often lacks direction. Teaching without them often is dull or irrelevant.

Of course the key to good research and scholarly inquiry is men—men who are good investigators and have scientific and scholarly imagination, ideas, zeal, and intellectual integrity. Pennsylvania has reason to be proud of the achievements of her scholars and scientists over the years. From the days of Joseph Leidy, Benjamin Rush, Edward Drinker Cope, and George Sharswood down to the more recent days when Bohlen, Cheyney, McMaster, Patten, Kuznets, Richards, and Schelling were here and active, great scholars and scientists have lent distinction to the faculty—and so they do today. Great advances have been made in the last fifty years. Under the present administration, which understands and encourages advanced study, even greater progress can be expected in the years immediately ahead.

But the central question is not whether many men are contributing to research and scholarly inquiry on the campus at Pennsylvania. The important question, in these days when problems and their complexity outpace men and their efforts to understand, is whether Pennsylvania is as good a host to such questing as she should be. And the answer has to be—as it would have to be in the case of any university—that she is not. There is vast room for improvement. The various studies of the Survey—plus the officers' comments—have pointed out certain facts and have tendered various suggestions for consideration.

Dr. Hayward Keniston, formerly Dean of the College of the University of Michigan, in connection with his report on Pennsylvania's Graduate School of Arts and Sciences made a study of the comparative scholarly standing of the various graduate disciplines and therefore, of the various universities in the country. In this study Pennsylvania ranked eleventh among the universities in the scholarly standing of her scholars and scientists who taught in the Graduate School of Arts and Sciences. (Pennsylvania's professional schools were not included. Their inclusion might have improved Pennsylvania's standing.) Dr. Keniston states the limitations of these judgment ratings in his study. Some of the ratings of Pennsylvania's major areas and some of the ratings of individual disciplines are very good; many are not so good as Pennsylvania would desire for the future.

Out of Pennsylvania's budget for academic expenses of $24,600,000 in 1957-1958, about seven and three quarters million was spent on research. This figure, of course, takes no account of individual faculty members who, without further financial support, spend much of their time on research or scholarly inquiry. Nor do these figures include funds spent on libraries and laboratories or for leaves of absence. The actual total for research and scholarly inquiry would be much more.

Pennsylvania's situation with regard to research and scholarly inquiry reveals three basic problems and a suggestion:

1. There is a great imbalance among the four basic areas in the amount of research support received. Seventy-seven per cent of the total sums from outside goes to medicine, biology, the physical sciences, and engineering.

2. The University's support of the work of individual scholars and scientists—out of its own funds—has been pitifully small. This figure has been raised substantially under President Harnwell's administration, but is still under $20,000 a year.

3. Of the total funds formally spent on research, only 8 per cent comes from the University's

budget—and is, therefore, to be considered as "hard money."

The suggestion is that it would seem appropriate for the University to add an item of overhead—as the University of Illinois is said to do—on all contracts with government and industry, this item to be used for the support of basic research and scholarly inquiry at the discretion of the University.

Survey studies led to a series of further suggestions concerning the administrative assistance to research and scholarly inquiry such as the following:

1. Given good scientific and scholarly talent on the faculty, one must place next an academic administration, from department chairman up to provost and president, which understands the place of basic studies, can identify good research and scholarly talent, and knows how to serve it without trying to run it. Such an administration might seek to provide: effective means of discovering and aiding talent; a moderate teaching load; a well-stocked and maintained main library reenforced by first class working libraries; a generous policy of part-or-full-time leaves of absence when a person of talent has a promising scholarly or scientific lead; freedom from excessive committee duty; adequate work places; adequate funds for the support of scientific and scholarly work.

2. A University Council on Research and Scholarly Inquiry might well be appointed—after consultation—by the President and Provost to advise on policy in the widest sense.

3. The University should work for a division of labor in advanced work among universities; to do this, Pennsylvania should mark out major areas for special emphasis.

4. A "Committee on Grants-in-aid to Individuals" (the present Committee on Advancement of Research) is obviously needed; the support available for such a committee should be substantially increased.

5. The Dean of the Graduate School of Arts and Sciences might well serve as an assistant to the President and Provost in the coordination of matters having to do with research and scholarly inquiry.

6. The degree of dependence on outside sources for the support of research poses many dangers—of independence, of consecutiveness of planning by the research person, of ethical effect on the recipient and of the character of research topics chosen; these dangers were spelled out in the Director's commentary and should be alertly watched.

7. To increase the amount of hard money and to facilitate the steady increase of funds devoted to the support of research and scholarly inquiry, a Pennsylvania Research Foundation is suggested;

8. The University might well be ready—when it discovers a person of rare ability who has a scholarly or scientific task that is in the grand tradition—to seek support for that person over a long period, perhaps on a ten- to twenty-year basis, so that the "cathedral" of his dreams may be realized.

A list of specific suggestions is less important than the spirit of the faculty, and of the administration toward research and scholarly inquiry. Great scholars and scientists are animated by a common passion—to extend the boundaries of knowledge. As one distinguished member of the university family has said:

> In earlier days research was a very personal affair. In spite of the extensive organization today of group effort, good research must have an intensely personal aspect. That is vital. The good research man feels the inherent beauty of the phenomena he is studying and the elegance of the means by which the object of his study is explored. He will have a sense of order and a passion for clarity. And he will have had very good training. His contagious spirit will communicate itself to his colleagues and especially to younger men.

To develop such men, to find others and to provide assistance and a congenial climate for their labors—this is a prime privilege of a university administration and of potential donors with the imagination to perceive the real monuments they can help to build. And the University should cherish the memories of those great men who have built and are building Pennsylvania's scientific and scholarly tradition.

B. *Pennsylvania as a Center for Graduate Education and Training in the Arts and Sciences*

The study of the Graduate School of Arts and Sciences was made by Dr. Hayward Keniston, formerly Dean of the College, University of Michigan. Most of the contents of this section is based on Dr. Keniston's excellent study. It has already been noted that in ratings of the faculty and program of the Graduate Schools of Arts and Sciences of the country, Pennsylvania stood eleventh among the universities of the country. In

commenting on the Pennsylvania Graduate School, Dr. Keniston says:

> While these ratings have no absolute validity, they do point to the conclusion that in the quality of its staff and the strengths of its programs of graduate study, Pennsylvania is one of the dozen outstanding institutions in the country. In the three quarters of a century since its founding it has become one of the great graduate centers. If it does not belong in the top group with Harvard, California, Columbia, Yale, Michigan and Chicago, it certainly is included in the second group, along with Princeton, Wisconsin, Cornell, Illinois, Minnesota and Stanford.

The issue of policy before Pennsylvania is this: "Should she, building on acknowledged strengths, aim towards having one of the truly great graduate schools? If the answer is 'Yes,' how does she go about it?"

Many opportunities for greater excellence are pointed out by Dr. Keniston in his excellent study.

The Graduate School of Arts and Sciences is the highest center concerned with advanced study and research. That work in the Graduate School may be and frequently is just as "professional" as work in the professional schools and that research in the best professional schools very often deals with the phenomena of their fields in a fashion quite as basic as that of any area in the Graduate School do not alter the fact that the Graduate School of Arts and Sciences, at its best, stands for love of learning in the widest and most disinterested sense. It, therefore, is the responsible agent for a great tradition. The Graduate School should be the apex of the intellectual life of the University and should feel an active concern that scholarship and research are being effectively fostered and that good teaching exists in graduate *and* professional schools.

But the Graduate School of Arts and Sciences does not realize its potential in most universities. Indeed, it too often is the "problem child"; and so it has been, in some degree, at Pennsylvania.

Where departments consist of strong persons who have high standards and are excellent teachers, who care about students and share with them the adventure of learning, great results follow. There are many such places now existing on the campus and the number is increasing. But the Graduate School of Arts and Sciences as a whole is not an effective organization. The reverse of the above strengths holds true in more areas than it should. The Graduate School has too little influence on the quality of education. It has almost no money; its total budget, apart from certain special grants that are channeled through it and certain fellowships which it administers, was at the conclusion of the Survey only $253,493. It has no headquarters for students—only a registration office. It keeps only a minimum of records—often not those which would best illuminate policy. Responsibility for admission of students (beyond requiring a bachelor's degree from an accredited institution) has rested with the departments. In 1959 this was changed so that the Graduate School determines admissions, on a basis of recommendations from departments. Many of the departments admit from 90 to 100 per cent of those who apply. Fellowships and scholarships do not compare in number and amount with those of similarly placed institutions. Candidates for the Ph.D. degree often drag on for many years before receiving their degree. In some departments undergraduate considerations dominate and lead to the giving of graduate courses by persons who belong more appropriately in undergraduate teaching. Instruction too frequently consists of formal lectures with little independent responsibility on the part of the student and too little sharing of the adventure of research and scholarly inquiry with students. The graduate student lacks a home and too often is the forgotten man. The Dean of the Graduate School holds an honorific post without power. These and other evidence of Pennsylvania's failure to make the most of her opportunities are cited by Keniston. It is difficult to say how much the reason lies in lack of power, lack of money, lack of initiative, lack of scholarly or scientific strength in certain departments, or just the inertia of tradition.

Reports by committees of the Association of American Graduate Schools indicate that dissatisfaction with the performance of many graduate schools is thus vigorously expressed.

The central cause of the lack of power in Pennsylvania's Graduate School of Arts and Sciences, as in the graduate schools of many universities, is that control over budget, the initiation of appointments and promotions, and, in general, the administration of departments, lies with the undergraduate and professional schools.

A responsible person in the university family recommended that all budgets of undergraduate schools should be submitted to the University administration through the Dean of the Graduate School of Arts and Sciences. To me, this does not seem wise. Nor did the idea that each department

be separated into two departments—one graduate and one undergraduate—win approval. It was believed that other effective ways of serving essential purposes could be found.

These "other effective ways" are suggested in the paragraphs which follow.

1. The Faculty: The quality of a Graduate School depends on the scholarly and scientific strength of the faculty and their interest and skill in teaching. Various suggestions for strengthening of the faculty were made: In certain areas, mature persons of established strength need to be added when finances permit. Able younger persons at the assistant professorial level will often prove better investments than older men with established reputations. They cost less—at the start—and have their major achievements ahead of them. The proportion of younger men should be increased. More attention should be given to recruiting from outside—with the best man picked wherever he is. The scholarly and scientific interests of young instructors should be carefully inquired into before appointment or promotion. A generous policy of leaves of absence to engage in research or scholarly inquiry will pay large dividends in the growth of men. To insure that in the consideration of appointments and promotions, balanced account is taken of the needs of the Graduate School, the following suggestions are offered: (*a*) The present practice of making the Dean of the Graduate School of Arts and Sciences sit on the University Committee on Appointments and Promotions should, of course, be continued. (*b*) The Dean of the Graduate School might well be asked to nominate representatives to the Provost, who, in turn might suggest one to the dean of each undergraduate school to serve on his Faculty Personnel Committee and speak for the needs of the Graduate School. (*c*) There should be gradually formed a larger nucleus of outstanding scholars and scientists, carefully selected, who will devote perhaps two-thirds of their time to graduate work.

2. The Students: The quality of the students taught is next in importance to that of the faculty. (If the problems of faculty are solved, many of the problems of student quality will solve themselves.) Among the suggestions offered by Keniston were the following: (*a*) The selection of graduate students should be *much* more selective. (*b*) More departments should adopt the policy followed by some of insisting on high quality and upon basic training in essential tools before admission, thus putting pressure on undergraduate colleges and schools to raise the level of their preparation. (*c*) If the quality of work is to be improved a much higher proportion of the graduate student body should be fulltime. (*d*) The search for talent should be vigorously pursued both outside and inside the University—as some departments are now doing. (*e*) The necessity for students to finance graduate study by a heavy teaching schedule of elementary classes may jeopardize their competence as future scholars and scientists. This problem awaits adequate fellowship aid so that the more promising of such men may have at least two consecutive years completely free for graduate study. (*f*) A further expanded fellowship program is desirable. Dr. Keniston suggests that stipends should be equal to those of the National Science Foundation.

3. A Graduate Center (including dormitories) is proposed to serve as a much needed social and intellectual headquarters for all graduate students—full-time and part-time. Such a center might resemble the Princeton Graduate Center or, even better, All Souls College of Oxford University. In such a center students might better help to educate each other. Full-time fellows and other full-time graduate students might have first claim on the Center's living quarters, to encourage the development of a community of high-quality students. A small number of carefully selected members of the younger or bachelor faculty might be appointed as "fellows" to live at the Center. In time scholarly men of affairs might become honorary fellows of such a Center—as at All Souls in Oxford. It is expected that more profound study, greater student responsibility, higher standards, greater readiness to play with ideas beyond the confines of a particular field—in short, a greater love and dedication to a life of learning—would grow from such a Center.

Supplementary facilities for tea and coffee to facilitate "bull sessions" in each main area of graduate study are also needed.

4. Of course, first-class strength in the library is vital to good graduate work. Collection of material must be pushed now if Pennsylvania is not to lose further ground. More adequate work places and seminar rooms should also be provided. This need will, in part only, be met by the new library.

5. The Programs of Work: Many suggestions were offered by Dr. Keniston and others concerning the educational work of the Graduate School

of Arts and Sciences, such as the following: (*a*) While the lecture system has its place, graduate education should not be primarily an imparting of knowledge by the teacher but an education into the mechanisms of scholarship and science; these can be taught more effectively by an apprenticeship system. (*b*) Significant improvement could be made if the number of courses could be substantially reduced and more time given to individual conference with students. (*c*) The statements by Dr. Robert MacLeod in his study of the undergraduate college, and the study by Dr. Bishop on honors systems in undergraduate education, could be consulted with profit by departments offering graduate work—so that individualization of education and the freedom and responsibility of students increased. (*d*) Credit-hour counting might well be superseded. (Dean Roy Nichols informs me that this has now been done.) A student should be encouraged to define his purpose in graduate study and then have the professor assist him in its advancement. (*e*) Every Ph.D. examining committee should include a member or members outside the department concerned, these persons to be nominated by the Quadrant Director to the Dean. (*f*) The taking of nearly all of a graduate student's courses in one department—a present practice of a few large departments—suggests that work is planned for the departments rather than for students. The interrelations of disciplines and the unity of learning are not served by this practice. (*g*) Graduate training through participation in research and scholarly inquiry with a faculty member is the ideal which should be pushed as far as practicable. (*h*) In the critical situation faced in the demand for more and better college teachers, Pennsylvania has a special opportunity and obligation to train graduate students in teaching—for herself as well as others. (*i*) If Dr. Keniston's suggested increase in the annual funds available for faculty research becomes available, it is proposed that the Dean of the Graduate School or the director of each quadrant systematically inquire into the research and scholarly inquiry needs of each member of the faculty and instructional staff. Where justified, increased funds should be available to the humanities and social sciences (including business and governmental administration) faculties.

6. The Administration of the Graduate School of Arts and Sciences: A series of suggestions were offered on administration to give the Dean, the Council of the Graduate School, and each quadrant faculty more power to intervene where such intervention would serve the cause of better standards. For example, it was suggested the Dean of the Graduate School might well sit on the University's budget committee. This would afford him a chance to study all budgets from the point of view of their provision for the needs of the Graduate School. The need of substantial funds for the Graduate School are desirable not only for the support of research and scholarly inquiry and for fellowships but also to finance graduate teaching when deans of undergraduate or professional schools hold back from financing such teaching by their own faculty members.

The above suggestions of small ways toward greater excellence in the Graduate School should be thought of only as illustrations of methods by which the Dean and faculty may work toward a still greater Graduate School at Pennsylvania. It should be noted that Dean Nichols is undertaking this task vigorously. It should further be noted that the university administration, by concentrating on the steps necessary to strengthen first one department and then another, is also moving vigorously to provide the basis for greater excellence at all levels. It was good fortune for Pennsylvania that, at the time when the Survey was pointing out situations and needs at Pennsylvania, there was an administration prepared to act vigorously in the interests of greater excellence.

The situation on the campus at Pennsylvania is promising; the Graduate School of Arts and Sciences seems headed toward a greater future.

C. *Other Areas of Education and Research*

Studies in a few other areas will be discussed, but more briefly than the preceding studies of research, of faculty, and of the Graduate School of Arts and Sciences.

In every case but one, each of the thirteen[4] professional schools—or schools with an acknowledged professional emphasis—was studied by an outsider or outsiders. The wealth of comment and suggestions was such as to be expected from scholars and scientists as eminent as our outside consultants. In one case—the School of Medicine—the study was directed by a member of the

[4] Counting the Engineering Schools as one. The new School of Communications was not studied.

Medical School Faculty—Dr. Dale Coman, Chairman of Pathology. This change was due to the illness and subsequent death of the original outside consultant, Dr. Alan Gregg, formerly Vice President of the Rockefeller Foundation. Dr. Robert F. Loeb of Columbia-Presbyterian and Dr. Stanley Dorst, of the University of Cincinnati served as advisers. In the case of the School of Veterinary Medicine, the School of Dentistry, the School of Nursing and the School of Education, the basic study was made by single outsiders—by Dr. Henrik J. Stafseth, of Michigan State University, Dr. Dean Willard C. Fleming of the Dental School of the University of California, Dr. Paul Woodring, formerly of the Western Washington State College and now of the Fund for the Advancement of Education, and Dr. Margaret Bridgman of the National School of Nursing, respectively. Where single individuals were not available to come full time, a Board of Consultants made the studies. A Board came for a series of visits of a number of days each and was supplemented usually by a Director of Studies from inside. This method was followed in the studies of the Wharton School, the Engineering Schools and the School of Fine Arts.

The details of each of these separate studies cannot be given attention here. Only a few general points will be possible.

Each of these studies offered many criticisms and suggestions as well as appreciation of strengths and achievements in Pennsylvania's professional schools. But each spoke plainly in order to be helpful. Certain general conclusions seem warranted:

1. Pennsylvania's widest drawing power has rested on her professional schools.

2. The needs of society for ever more and more highly trained professional knowledge and skills will lead to demands for professional education and training in additional fields. To me, one of the most compelling examples is in study and teaching in the problems of elementary and secondary education.[5] Universities have either ignored this major responsibility or dealt with it by their left hand. Policy should not be determined by unresolved tugs for power.

It is my personal judgment based on the studies of the Survey that the following policies might well control in the consideration of expansion into new fields.

It cannot well be assumed that the world's future need—and Pennsylvania's future obligation—are completely covered by the existing professional schools.

In considering the establishment of a new professional school, it might be well to examine the following considerations:

The financing of a new school should be clearly enough in sight to insure that the present schools of strength and importance and unmet need will not be impoverished by the future needs of the latest newcomer. The advice of a wise man is repeated here: "A university is distinguished by what it does superbly—*and* by what it refuses to do."

The first obligation is to increase the excellence of the basic professional schools, for which responsibility has already been assumed.

To be accepted as a proper subject for professional education in the University, any field proposed should not only be a response to a substantial and durable demand in society for specialized competence in the area, but should possess or give convincing promise of acquiring the following characteristics: A body of established and communicable knowledge. Research opportunities and active capacities to insure steady expansion of that knowledge. The necessity for formal thinking and training in basic fields as well as applied ones; for this it is necessary to have or to offer the possibility of attaining a theoretical or conceptual structure. A dedication to the public service that may be expressed in a formal code or may be in process of emerging as approved practice. Public sanctions (in some cases).

I am indebted to a committee of Columbia University on "The Education for the Professions," Professor Elliott Cheatham, Chairman, for helpful ideas. These bear on the educational implications of the setting in which graduates of professional schools will function.

The Cheatham report stresses that:[6]

1. The tradition that mankind is to be trained to adjust to fixed conditions is no longer tenable. Professional men must be prepared to adapt to

[5] Dr. Paul Woodring of the Fund for the Advancement of Education laid out the lines of advance excellently for Pennsylvania in this field.

[6] Quotations are often not precise; quotes, therefore, are not used in such cases. The ideas come from Professor Cheatham's committee's report.

novel circumstances from the beginning to the end of their careers.

2. The store of knowledge is so immense, the expansion of knowledge so rapid and the rate of obsolescence in applied knowledge so swift that much of the factual knowledge and applications acquired in a university may be quickly outmoded.

3. The present exaggerated emphasis on the acquiring of information that is not basic is, therefore, a misplaced emphasis.[7]

4. The aim of education should be to prepare professional students in "disciplined intellectual power and attitude to acquire new knowledge through life."

5. Education to develop "disciplined intellectual power" connotes an active and constant understanding of the contrasts between: intellectual discipline and information; accuracy of comprehension and precision of expression on the one hand and vagueness and generality on the other; intellectual grasp and rote memorization; thoroughness in dealing with a few subjects and less deep plowing in many; principles and specialized information; teaching that enables a student to acquire and develop new knowledge on his own and that which merely provides information from others; completeness of curriculum offering and adequacy of individual education.

In addition to these requirements in professional education, a modern profession requires the widest possible foundation in *good* liberal education. The education of the professional student, from the time he enters as a freshman until he attains a graduate professional degree, needs to be examined as a whole, and experiments in search of improvements sought on each side of the total task.

It is with regret that the problems of undergraduate education will be passed over in this discussion. They are not passed over because of any sense of their being of lesser importance. They are not. But adequate discussion would require another speech; this disquisition has been long enough. But one further matter will be considered.

[7] This is not intended to deny that students in any area, especially in professional fields, must gain an immense body of knowledge—factual knowledge—as a basis for their work. It is axiomatic that one can't think unless he has something to think about. The objection here is to ephemeral information rather than to basic knowledge, and to information unrelated to a context of ideas.

ORGANIZATION, ADMINISTRATION, AND GOVERNMENT

This further glimpse of the work of the Educational Survey will concern itself with the Board of Trustees.

How to make faculty, administration, and Board of Trustees an effective triumvirate in the service of excellence in a university is a problem which varies with every university. How each may appropriately perform its special function in the service of excellence is a happy conclusion not always realized.

At the start of the Survey it had been understood with President Harnwell that the Survey was free to study anything which affected education and research. When, under these terms, the question was raised of a study of the Board of Trustees, the Board voted unanimously in favor of such a study on the ground that it would be inappropriate to ask the faculty to be studied and for the trustees to refuse to be studied.

After a five-months' search, Mr. Donald R. Belcher, formerly Treasurer of the American Telephone and Telegraph Company and Assistant Director of the United States Bureau of the Budget [8] was invited to make the study, accepted and spent a year in the study.

Mr. Belcher's study was a cooperative study. Each member of the Board was considered a member of an advisory committee and twice was asked to read Mr. Belcher's report in draft and criticize it. These criticisms were carefully considered by Mr. Belcher. Three dinner meetings were held with an invited guest at each to speak and deal in a discussion of effective Trusteeship as practiced at his institution.

The guests at these three meetings were Mr. Charles Coolidge, Senior Fellow of the Harvard Corporation; Mr. Laird Bell, longtime Chairman of the Board of Trustees of the University of Chicago; Dr. Harold Dodds, formerly President and Trustee of Princeton University. Each of these spoke of the functions of Trusteeship, of the proper limits to those functions and their proper relation to the President and Faculty.

Mr. Belcher's excellent report covered the whole gamut of Trusteeship responsibilities and problems. He carefully defined the responsibili-

[8] Mr. Belcher had spent nine years at the beginning of his life as a teacher of mathematics at Hanover College and at Columbia University—and two years after retirement from A.T. & T. as Regents Professor at the University of California.

ties of trustees, administration and faculty and the relationship among the three. He outlined the ways in which Trustees may really serve the excellence of the institution and the areas which they should not invade.

Mr. Belcher further recommended: a retirement age; a limitation of tenure for Term and Alumni Trustees; selection of new members from a younger age bracket; inclusion of more non-alumni, more women and more educational leaders of quality from other institutions.

Mr. Belcher's report also offered many suggestions as to ways in which the functioning of the Board as an organization might be improved. Similarly suggestions were offered for the increase of effectiveness of Advisory Boards of Trustees. He defined the desirable characteristics of a new trustee. He stressed the importance of trustees being given a sense of participation and the ways by which this can best be done. He pointed out that faculty members are not to be considered as employees of a corporation. He avoided hard and fast rules. A spirit of mutual trust and confidence coupled with a wholesome respect of each group (Board and Faculty) for the prerogatives of the other was essential. "A good tradition is better than any law." Mr. Belcher concludes:

> To preserve the traditions that are worthy of preservation and at the same time with vision and courage to make the changes which problems of our present-day democracy require—these constitute the task of the Board of Trustees no less than of its educational and administrative associates. Surely there are few opportunities for public service more intriguing than that of helping to shape the future of an institution that is destined to train and inspire the youth of coming generations. Trusteeship in the University of Pennsylvania is a great and noble privilege.

Upon the submission of the Belcher report, the Chairman of the Board, Dr. Alfred H. Williams, promptly appointed a strong committee to consider its assumptions and suggestions. Many of the suggestions required no action but were suggestions for the attention of administration. But the suggestions calling for action by the Board were promptly considered and usually adopted. Sometimes the Board went further than the Belcher recommendations. For example, Mr. Belcher proposed to change the absence of any retiring age by substituting a retiring age of seventy-two. The Board reduced this to seventy (for members not now holding life tenure) and voted to create the position of trustee emeritus. A number of members immediately applied for such status, which permits all the privileges of membership except the right to vote. This made it possible to elect younger men as trustees—men who might otherwise have accepted membership and made commitments to other institutions and thus become lost to Pennsylvania.

As in the case of each other study, the educational results of the Belcher study were the most important. In my judgment, the three members of the triumvirate which governs Pennsylvania, have, as a result of this study and others, a better understanding of and respect for each other's responsibilities and their limits.

IN CONCLUSION

These glimpses into the studies of the Educational Survey may suggest to your imagination, something of the nature of the enterprise and its effects. But I am sure your greatest respect will be for the Board, Administration and Faculty who invited such an uncomfortable business and, with some minor exceptions, cooperated with it and faced up to the issues raised. I repeat that no good would have come of this if the President of the University had not been a man of great executive power, a man who gets things done, a man dedicated to the University and pledged to the policy of cooperation and consultation with the faculty, a man who focuses attention on points needing attention and who saw to it that Survey studies were considered. Something of the power of this administration and its friends and supporters is evident from the fact that the annual "academic" budget had increased between 1953 and 1961 by 86 per cent.[9]

Something of the spirit which has come into being on the campus is suggested in the words of Dr. Jonathan Rhoads in his concluding report as Provost of the University. He wrote as follows:

> Pennsylvania is a great university but, fortunately, a dissatisfied one. Its further development is bounded by horizons and not by walls. It will be the sum or at least the resultant of the efforts of the individuals who compose it. They have in common the desire to

[9] The *academic* budget was $23,700,000 in 1952–1953. It had increased by $20,000,000 in 1960–1961, an increase of 86 per cent. The *total* budget of the University in 1952–1953 was $27,263,000. By 1960–1961, it had increased by $21,340,000, or 78 per cent.

develop a greater university here at Pennsylvania than this country has yet known. It will take intense and sustained effort, but I see nothing to prevent success—and within our time.

These words come from a man who is not at all flamboyant. They are significant of a more hopeful and adventurous spirit on the campus.

In conclusion, I again acknowledge the debt of the Survey to so many people within and without the University. They have made the Survey. I do not know of another case where there has been such a release of creative energy addressed to the problems of a single university. To have had the chance to learn from all these participants about the age-old competition between excellence and mediocrity in a university has been an inestimable privilege.

Should any of you be led by this example to undertake a similar adventure in your own favorite university, I wish to warn you of one thing: One such immersion will be quite enough to serve you for the rest of your days.

THE DEMAND FOR MONEY

MILTON FRIEDMAN

Professor of Economics, University of Chicago, and
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(*Read November 10, 1960*)

THE term "money" is ordinarily used in two quite different senses. We speak of a man having "money" in his pocket, and we also speak of him as "making money," even when he is not a counterfeiter. I shall use the term solely in the first sense, since the term "income" is available for the second.

The economic function of "money" in this sense is to permit exchange without barter, to enable an individual to exchange the goods or services he owns for other goods or services he wishes to consume or to hold without having to match up each transaction. Instead he can sell at one time to one set of individuals for generalized purchasing power and buy at a different time from other individuals by drawing on his stock of generalized purchasing power. Money in this sense consists of anything that serves the function of providing a temporary abode for general purchasing power.

Money has at times consisted of wampum, stones, cigarettes, cognac, and, of course, such precious metals as gold and silver. Today it consists mostly of less tangible stuff, of promises to pay in the form of the pieces of paper we carry in our pockets and the entries on the books of banks that are misleadingly labeled deposits. It is somewhat arbitrary and a matter of considerable dispute just where it is best to draw the line between the promises to pay that are termed "money" and other very similar promises to pay that are termed "near-moneys" or "liquid assets." For the purposes of this paper, I shall designate as money the non-banking public's holdings of currency and all deposits, demand and time, in commercial banks.[1]

Each of us separately can make the amount of money in his possession anything he wishes subject to the limits imposed by his total wealth and borrowing capacity. If at any moment of time, he wishes to hold more cash, he can sell other assets, or borrow; if he wishes to hold less, he can buy other assets, or repay debts. Over time, of course, he can also add to his cash balances by spending less than his receipts, and subtract from his cash balances by spending more than his receipts.

All of us together, however, are in a very different position. Broadly speaking, the total amount of money available to be held by all together is determined by the monetary structure, or the monetary authorities—currently in this country, the Federal Reserve System—largely independently of the actions of the holders of money. Under these circumstances, one man can add to his cash balances only because somebody else reduces his. If all of us together were to try to reduce or add to our cash balances, we could not do so; somebody or other would have to hold the amount of money and no more than the amount of money that there is available to hold. We would all of us, as it were, simply be playing a game of musical chairs.

For example, an attempt by all of us to reduce our cash balances would mean that we were all of us trying to spend on assets (such as houses and bonds) and on current consumption more than we were simultaneously receiving from the sale of assets and current services. But this is clearly impossible since one man's expenditures are another man's receipts and double entry books must balance. However, the attempt would have important effects. It would produce spirited bidding for assets and other goods which would raise their prices. At higher prices, the same number of dollars would correspond to a smaller total of goods and services. There would be some price level at which the community would no longer wish to reduce its cash balances. Although frustrated in the attempt to change the number of dollars of money held, the community would have succeeded in reducing the amount of cash balances expressed in terms of goods and services, its "real" balances, as they are generally termed.

Conversely, an attempt by all of us to add to our cash balances would mean an attempt to spend

[1] In more technical terminology, currency outside banks plus demand deposits adjusted plus time deposits in commercial banks.

less than we were receiving which would tend to mean a decline in total expenditures and in prices. Nominal balances would remain the same but real balances would rise.

In short, the public cannot determine the nominal amount of money, but it can make the real amount anything it wishes to by bidding prices up or down, in the process raising or lowering money income. This essential difference between the situation as it appears to the individual, who can determine his own cash balances but must take prices and money income as beyond his control, and the situation as it is to all individuals together, whose total cash balances are outside their control but who can determine prices and money income, is perhaps the most important proposition in monetary theory and certainly the source of greatest confusion to the layman. It is also what gives such importance to the subject matter of the present paper, namely, the factors that determine the amount of cash balances the public wishes to hold.

This topic is an ancient one, of course, and abstract analyses date back several centuries. Quantitative analyses have been much rarer, for the usual reason that satisfactory data covering a sufficiently long period have been lacking. In the past few years, two sets of data have become available from studies of the National Bureau of Economic Research which permit a far more searching empirical analysis than has heretofore been possible of the factors determining the amount of its wealth that the public wishes to hold in the form of money. One, directly relevant to monetary analysis, is a series of comprehensive estimates of the stock of money in the United States covering nearly the past century, constructed by Anna J. Schwartz and myself. The other, relevant to a much wider range of studies, is a series of estimates of national income covering nearly the same period, constructed by Simon Kuznets.

The income estimates are important for a study of the demand for money because the amount of money demanded clearly depends in the first instance on how much exchange there is for money to mediate, which is to say, on the volume of goods and services to be exchanged and the prices at which they exchange. Total national income is one index and perhaps the best single index, of the amount of exchange to be effected. Consequently, we can allow for the size of the economy by expressing the stock of money as a ratio to the flow of income, measuring money stocks in terms of the number of months of income to which they are equivalent (or, as is perhaps more usual, using the reciprocal of this ratio, the income velocity of circulation).

When the money stock is expressed in these terms, the most striking feature of its behavior over the past century is its fairly steady and persistent rise (chart 1). Around 1880 the public at large, including individuals, all business enterprises other than banks, and all governmental units other than the Federal Treasury, held a stock of money equal in value to about two-and-one-half month's income. Today, the public's stock of money corresponds to more than seven months' income, or nearly three times as much.

The most plausible explanation of this rise in the stock of money is that it reflects the associated quintupling of per capita real income, which is to say, of the average level of living. With a rise in the level of real income, people may want to increase their stock of money more or less than proportionately, just as a rise in level of living means a less than proportionate increase in expenditures on bread but a more than proportionate increase in the stock of durable consumer goods. Judged by the long-period evidence, money is in this respect a "luxury" like durable consumer goods, rather than a "necessity" like bread. A one per cent increase in real per capita income has on the average been associated with an increase in real per capita money

holdings of about one and two-thirds per cent.

The general covariation of money, expressed as a ratio to income, and of per capita real income over the past nine decades in the United States is not by itself very strong evidence that changes in the one are a consequence of changes in the other. The covariation might be coincidence, or it might be attributable to common third factors. And, indeed, some recent work has attributed the rise in the money-income ratio to a decline in interest rates rather than to a rise in real per capita income.[2] This explanation, too, is plausible on abstract grounds. The lower the interest rate, the less income is sacrificed by holding wealth in the form of money rather than in the form (say) of bonds, hence the larger the fraction of their wealth that people would tend to hold in that form. However, a number of pieces of empirical evidence indicate rather strongly that the rise in per capita real income is far more important a factor explaining the rise in the money-income ratio than is any change in interest rates. First, the covariation of the money-income ratio with real per capita income has obtained for a much longer period of time than the covariation with interest rates. Both the monetary ratio and real per capita income have risen pretty regularly over the past nine decades, whereas interest rates have been constant or rising over long stretches of that period. Second, for a number of foreign countries for which data are available, the covariation between the money-income ratio and real per capita income has been in the same direction and of roughly the same magnitude as in the United States, though changes in income have not always borne the same relation to changes in interest rates as in the United States. Third, data on deposits per capita and income per capita are available for different States in the United States. These cross-section data display roughly the same relation as the time-series data.

One reason why uncertainty attaches to the meaning of the longer-period relation is because a very different relation prevails for the shorter periods corresponding to business cycles. The money-income ratio generally falls during cyclical expansion when real per capita income is rising and generally rises during cyclical contraction when income is falling. Over cycles, therefore, the money-income ratio and real per capita income move in opposite directions whereas over long periods they move in the same direction.

The explanation for this apparent contradiction that currently seems to me most plausible exemplifies the tendency so often noted in science for results obtained in one connection to have applications in other wholly unexpected connections, for the same kind of formal problem to arise in very different substantive contexts. In work that I did some time ago on consumer spending and saving, I developed a hypothesis (partly to explain apparent contradictions in that field between time-series studies and cross-section studies) which had as its central feature a distinction between income as recorded by statisticians, which I called measured income, and a longer term concept of income to which consumers adjust their spending, which I called permanent income.[3] It turns out that this same distinction, applied to prices as well as to income, can reconcile the apparent contradiction between the cyclical and secular behavior of the money-income ratio.

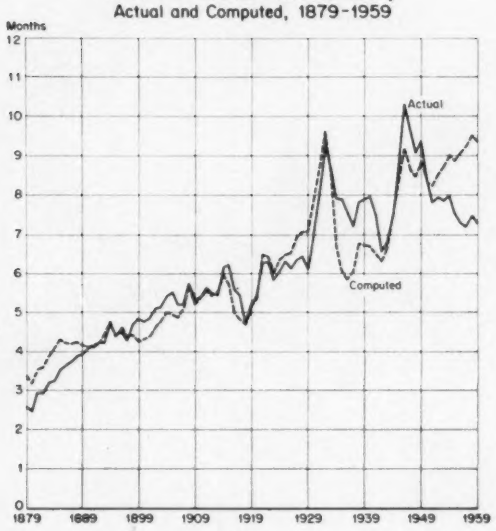
Let us suppose that holders of money, whether individuals or business enterprises, adjust their holdings not to their current receipts or current prices but to the receipts and prices they expect to prevail over a somewhat longer future period. This difference will be of little significance for long-period data; over decades, the expected or permanent magnitudes will move in the same direction as the measured magnitudes. But the difference will be important over cycles. In a cyclical expansion, measured income will presumably rise decidedly more than permanent income. The stock of money might therefore rise more than in proportion to permanent income, which is what it does over longer periods, yet less than in proportion to measured income, which is what it does over cycles. The distinction between the two concepts of income in this way can provide a reconciliation of the cyclical and secular results.

A statistical analysis based on this distinction, initially computed for the period 1873–1954, gave extremely good results. These computations used not only the general ideas just outlined that were derived from the consumption study but also the

[2] See Latane, Henry A., Income velocity and interest rates—a pragmatic approach, *Hearings on Employment, Growth and Price Levels* before Joint Economic Committee, Congress of the U.S., Part 10: 3435–3443, Washington, 1960.

[3] Friedman, Milton, *A theory of the consumption function*, Princeton, National Bureau of Economic Research, 1957.

Chart 2
Months of Income Held as Money
Actual and Computed, 1879-1959



supplementary notion, which, interestingly enough, had as one of its origins a study of hyperinflations,[4] that expected income could be estimated as a weighted average of past measured incomes, the weights declining exponentially. Moreover, the numerical values of the weights derived in the consumption study were carried over to the money computations. For the rest, the computations were based entirely on averages for whole business cycles, that is, for the period running from the trough of one cycle to the trough of the next, or from the peak of one cycle to the peak of the next. From such cycle averages we estimated an equation connecting the actual stock of money held per capita, adjusted for changes in expected prices, with expected real income per capita. We then used this equation, plus the yearly values of prices and income, to compute the hypothetical amounts of money that would have been demanded if the public had reacted precisely in accordance with our estimated equation. The resulting hypothetical money-income ratio is shown in chart 2 along with the actual. The most striking feature of the results is not the general concordance but rather the agreement between the cyclical movements of the two series despite the fact that no use was made of intracyclical movements in deriving the statistical relation.[5]

That a fairly simple relation of this kind approximates actual experience over a century and from year to year as well as decade to decade is striking testimony to the fundamental persistence of essentially unchanged behavior patterns with respect to money holdings, behavior patterns that have determined the public's response to the great changes occurring in the economy's monetary circumstances.

Despite the generally good fit of the statistical relation, there are a number of sizable discrepancies. One is for the early years of our period, from about 1870 to 1890. The money and income data are least accurate for this period and there is external evidence that this may well explain the discrepancy. A much more disturbing discrepancy is for the period after World War II, when the money-income ratio fell decidedly relative to the level that would have been expected on the basis of our statistical relation. This discrepancy was already notable when the analysis just described was completed and published. It became even wider in the next few years as additional data became available.

One striking feature about the discrepancy is that it is present only in the major postwar movements in the money-income ratio, not in its cyclical movements. Our statistical relation continues to reproduce with a high degree of fidelity the cyclical movements in the money-income ratio. The discrepancy does not therefore raise any doubts about the importance of the distinction between measured and permanent magnitudes.

The phenomenon underlying the discrepancy is that over the postwar period money balances have fallen relative to income (velocity has risen), whereas, as we have seen, from the Civil War to World War II money balances tended to rise relative to income. This phenomenon has attracted much attention and numerous explanations have been suggested. The most plausible attribute the postwar decline in the money-income ratio to the postwar rise in interest rates, or to expectations of inflation, or to the growth of savings and loan shares and other substitutes for money, or to some combination of all three factors. It may be that

[4] Cagan, Phillip, The monetary dynamics of hyperinflation, in *Studies in the quantity theory of money*, edited by Milton Friedman, 37–39, Chicago, 1956.

[5] For a full exposition, see Friedman, Milton, The demand for money: some theoretical and empirical results, *Occasional Paper No. 68*, National Bureau of Economic Research, 1959, reprinted from *Journal of Political Economy*, August 1959.

these factors account for some part of the postwar decline in the money-income ratio. However, I am myself persuaded, on the basis of a more intensive examination of the evidence than time permits detailing here, that even all together they can account at most for only a minor part of the discrepancy.

A more promising explanation that I am currently investigating is that the discrepancy reflects a very different factor, namely, the public's expectations about the likely degree of economic stability. This explanation has great appeal both on analytical grounds and in terms of its qualitative concordance with the experience of the past four decades. Given that money is held as a temporary abode for generalized purchasing power, the amount people will desire to hold must surely depend on their expectations about future contingencies. If they anticipate a highly stable world, with minor fluctuations in income, employment, interest rates, and prices, they will feel much less of a need to retain relatively large amounts of their wealth in the form of money than if they anticipate considerable instability, involving wide fluctuations in income, employment, interest rates, and prices.

The explanation accords qualitatively with the behavior of the money-income ratio over the past four decades. The 'twenties with its widespread belief that a "new era" had dawned was a period when, on this interpretation, people might have been expected to hold relatively low cash balances and, in fact, the money-income ratio is low during this period relative to its long period trend. The enormous increase in uncertainty after the 1929 crash was promptly accompanied by a sharp increase in money balances relative to income. While nominal money balances declined sharply as a result of the unfortunate policies followed by the monetary authorities, income fell still more sharply as a result of the reaction of the public, so that money balances rose sharply relative to income. Money balances remained high relative to income throughout the uncertain 'thirties, and fell only as the approach and then outbreak of war in Europe quieted fears of continued economic depression. After the United States entered the war, and even more as the end of the war approached, expectations again shifted. The experience of the 'thirties combined with the recollection of the collapse of prices after World War I to instill widespread uncertainty about postwar experience. Money balances rose to an unprecedentedly high level relative to income. Finally, and here we come to the period that has attracted most attention, after the end of the war, as one recession after another turned out to be mild, fears of a great depression receded, confidence grew in the continuation of a high and relatively stable level of economic activity, and cash balances fell substantially relative to income. This postwar change in expectations must be about over. Hence, if this interpretation is correct, the factors accounting for the postwar decline in the money-income ratio have spent their force, and the ratio may be expected to resume its long-period upward trend. Whether it does so will be a critical test of the interpretation.

This qualitative account suggests that changes in the public's expectations about economic instability were in the right direction to account for the major deviations of the money-income ratio from its historical relation to expected income. It leaves open, however, the question whether such changes in expectation could account for the magnitude of the observed deviations. In order to get some evidence on this point, I have been trying to supplement the qualitative analysis by a quantitative analysis. The results of my initial experiments are mixed. On the one hand, the introduction of a measure of expected instability analogous to the measure of expected income accounts, in a statistical sense, for a large part of the deviations since the early 1930's. On the other hand, the measure in its present form makes the fit of our relation to experience decidely worse for the period prior to 1920. Clearly, much further work is needed before we can tell whether this promising lead will turn out to be a dead end or to open up new vistas.

It should be emphasized that I have been reporting on research in progress and that the findings reported are still tentative. This is especially so with respect to the role assigned to expectations about economic stability, and even more, the particular device adopted to get an empirical measure of such expectations. This variable and this device have not heretofore been used, so far as I know, in this or any other comparable connection. Hence, even if they should turn out to give good results for the United States, we must await further applications to data for other countries and to other problems of a similar character before we can have substantial confidence that favorable results in the present instance are more than a happy accident.

Another respect in which our work is tentative is that we have so far not been able to isolate the effect in the United States of a set of variables that occupy a central place in the theoretical analysis of the demand for money, namely, variables determining the cost of holding money in the form of cash rather than of other assets. Interest rates measure this cost if the alternative is a bond or loan; the rate of change of prices measures this cost if the alternative is physical commodities. Under circumstances where these costs have varied widely, as for example in hyperinflation, their effect is clear and manifest, so experience supports theoretical expectations. In the United States, on the other hand, these costs have apparently varied over a sufficiently narrow range that their effects are hard to disentangle from the effect of the myriad other factors that influence desired cash balances and that we label "chance." Perhaps, once we have determined fairly accurately the effects of what seem to be the much more important variables of expected real income and expected stability of income, we shall be able to isolate more satisfactorily the effects of interest rates and rates of change of prices.

The stakes in these investigations are high. If the factors governing money-holding propensities are known, and if these effects are highly stable, we shall be armed with a powerful apparatus for the amelioration of what has been for centuries a basic problem of economic life: the fluctuating value of the monetary unit.

THE EVOLUTIONARY ANTHROPOLOGISTS: THE PROGRESS OF MANKIND

The Concepts of Progress and Culture in the Thought of John Lubbock, Edward B. Tylor, and Lewis H. Morgan

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> Whether the naturalist believes in the views given by Lamarck, by Geoffroy St. Hilaire, by the author of the "Vestiges," by Mr. Wallace and myself, or in any other such view, signifies extremely little in comparison with the admission that species have descended from other species, and have not been created immutable; for he who admits this as a great truth has a wide field opened to him for further inquiry.
>
> —Charles Darwin, in the *Athenaeum*.

In the French town of Abbeville there stands a statue in memory of a local crank, Jacques Boucher de Crèvecœur de Perthes. This nineteenth-century eccentric insisted that he had found stone tools which were shaped by human hands before the time of the Biblical flood. No one believed him despite the fact that his claims were in the orthodox tradition that man existed before the Deluge. Perhaps it was the crudeness of the technology and the image of the men who used such tools that was so unsettling to nineteenth-century eyes. No one doubted the sincerity of de Perthes, only his sanity and geology. Like an old refrain when the wise always know best was the dismissal of his so-called "haches antédiluviennes."

The picture became more clouded with the unearthing of human artifacts together with fossils of extinct animals, but the prevailing view was not to be challenged by evidence which could be interpreted away. If de Perthes publicized his findings and spelled out his preposterous inferences in book after book (six by 1847), these were the ravings of a man possessed. The first sympathetic treatment which de Perthes received came from a man who set out to silence him, Dr. Rigollot of Amiens. By the time that Rigollot had finished digging in the gravel pits of Saint Acheul, he was on de Perthes' side. Rigollot kept finding flint axes along with strange fossils too.

In 1908, seventy years after de Perthes claimed that man had kept company with extinct mammals, the local professional society which had scoffed and turned deaf ears to de Perthes' fantasies erected his memorial statue in Abbeville. Things stood in a different light. In the intervening years, enthusiasm for science had become almost a middle-class fad in England, and "mechanics' institutes" attracted crowded audiences to hear distinguished speakers, even when their subject was geology. Huxley found a ready audience in Edinburgh for his popular lectures on man's kinship with other animals. Lyell was elected to the faculty of King's College by a board stacked with bishops. There he instructed gentlemen as well as ladies in the ways of uniformitarianism, of course with consummate tact. Queen Victoria knew his *Antiquity of Man* and cross-examined Lyell at Balmoral about caves, rivers, and fossils the year that his book was published.

Perhaps it is true that even the clerics no longer believed the Genesis account of creation; at least Archbishop Usher's chronology, dating the year of the creation as 4004 B.C., had become an antiquarian curiosity. The Stone, Bronze, and Iron Ages of the Scandinavian archaeologists, postulated before de Perthes' heresy, had fixed a scheme of successive technological stages which had become a model of evolution. The "Celtic" tools which de Perthes had once claimed to be pre-Roman turned out to be Neolithic. And Darwin had found a hypothesis to account for "the origin of species" and "the descent of man" by natural selection with a mass of evidence to support his claims. Darwin and de Perthes were contemporaries, and British colleagues of Darwin did not find the Frenchman a mad man. In fact, in the year of *The Origin*, two of Britain's foremost geologists vouched for de Perthes to the high courts of British science, Joseph Prestwich

to the Royal Society and Lyell to the British Association for the Advancement of Science.

Before he was done, Lyell went over the ground around Abbeville and examined de Perthes' discoveries three separate times. Lyell was cautious and circumspect, a professional barrister as well as one trained in the pitfalls of assessing geological data; if he tried Darwin's patience by hanging back in confirming the new biological theory, when Lyell spoke, people listened. Without noticeable enthusiasm but compelled by the evidence, Lyell finally joined the chorus of Darwin's supporters—but not before he had prepared the way for Darwin in his own *Principles of Geology* and had become a convert of de Perthes' strange faith.

The coincidence of the publication of Darwin's *The Origin of Species* (1859) and Boucher de Perthes' *De l'Homme Antédiluvienne et de ses Œuvres* (1860) presages the inauguration of modern anthropology. By 1871, almost within a decade, the principal works of Sir Henry Maine, E. B. Tylor, J. F. McLennan, and Sir John Lubbock had appeared in England; and Lewis H. Morgan's *Systems of Consanguinity and Affinity of the Human Family*, an evolutionary landmark based on kinship systems, had been published by the Smithsonian Institution in the United States. This was the beginning of the evolutionary era in anthropology, the period in which the "development hypothesis" became the guiding principle in social analysis, for the men who gave that era its characteristic bias and working ideas were committed to the evolutionary idea.

Theirs was the same task which Huxley set himself in *Man's Place in Nature* (1863) except, of course, the anthropologists studied human institutions, practices, and technology. In their hands, man the social animal was to find his place in the same scheme of nature where Huxley's and Darwin's researches had located him zoologically. Huxley's bout over Neanderthal man, when the status of this apelike creature was much disputed, had not put Huxley on the side of the angels. Neanderthal man—whose flat head, low forehead, and large brow-ridges stamped him as primitive and human—was not merely compatible with ancient flint workers who practiced their technological skills in the time of European mammoth and rhinoceros; he was one of those Pleistocene craftsman. Here was the "man" whom Huxley refused to dismiss as either a pathological idiot or a degenerated Adam; he was a kinsman of that gregarious primate of the anthropologists (suborder Anthropoidea and family Hominidae) who lumbered his unsteady way through the evolutionary maze from savagery to civilization.

I. DARWIN'S RELATION TO THE NEW ANTHROPOLOGY

Two figures stand out among the cultural anthropologists, Edward B. Tylor (1832–1917) and Lewis Henry Morgan (1818–1881), and on all sides they are acclaimed the giants of the period. John Lubbock (1834–1913), although a lesser figure, deserves attention on another score, for his life reflected many evolutionary currents in a particularly vivid way. He is a personal reminder of how closely aligned were the new science of anthropology and those influences which figured sometimes directly and sometimes indirectly in its coming of age. To call these men "Darwinians" is suggestive and at the same time not wholly accurate. They were caught up in a movement of which biological Darwinism was the most publicized center; and while "Darwinian" is a convenient label, more apt than most such labels usually are, it is not without some awkwardness. The anthropologists were not outright and literal Darwinians, and they were even less disciples of the evolutionary philosopher Herbert Spencer. Progress was not identical with evolution and could not, as Spencer insisted, be equated with it.

When the romanticized "typical" Victorian is recalled, he is sure to be endowed with a complacent, provincial optimism and an almost religious faith in progress. He may not know exactly what he means by "progress," but he is certain that there is such a thing and that its beneficent wonders are self-evident to all right-thinking men. The ethnologists do not fit this stereotype. To be sure, they were all unashamedly on the side of progress, but they were less giddy and more discriminating than they are usually given credit for. They suffered the excesses of enthusiasm for things close to home and for the familiar articles of nineteenth-century life, including monogamous marriage. Morgan thought of the Eskimos as downright "peculiar," and he voiced a widespread myth when he claimed a certain moral and intellectual superiority for "Aryan" ways. But even Lubbock, whose revulsion at the "perversity" of savages is more notorious than Morgan's, was not so blinded by progress and so smug that he supposed his

world would not be surpassed. He was too convinced of the reality of progress and the need of improvement. However high the pinnacle of development, this peak was sure to be topped by the process that had carried the world this far.

Progress, whatever else it was, was a future promise. Progress opened new prospects and did not merely represent the solid achievements of the present. Tylor looked down the corridor in an opposite direction and shrewdly saw that "taking something away" was as true a mark of progress as adding something new. Men advanced by dropping certain customs, kinds of conduct heinous enough to make their mere disappearance a triumph. Tylor could also chide the fanatical optimist by remarking that the world may not have been constructed "for the purpose of providing for the human mind just what it is pleased to ask for."

In judging the Victorian ethnologists, we should not forget that they were embattled men straining to take the curse of Adam from their primeval ancestors. They recognized that progress was a human creation due to man's natural capacity as a social animal. As much as anything else, their contention of human progress was a vote of confidence for the species against those who insisted that if evolutionary man was a wayward ape, he would have been doomed to brutishness. In all the loose talk about progress, the evolutionary anthropologists worked their way toward the view that a sequence of development imposed its own logic on the ordering of events, and that later stages presupposed earlier ones without which the later were impossible. If they misconstrued the logic, they held soundly to the evolutionary principle that the chain of events ushered the new from the old.

The relation of biological to cultural evolution in the history of ideas is complex. In Germany, Adolf Bastian (1826–1905) could repudiate biological Darwinism and at the same time be a cultural evolutionist. The Swiss J. J. Bachofen (1815–1887), author of *Das Mutterrecht*, could use the principle of "survivals" and write of progressive cultural stages without any indication of Darwin's influence. Conceivably, Darwin's impact might never have been felt, and Tylor, Morgan, Lubbock, and the rest could have gone about their work with similar results. The stubborn fact remains that most of them did not; and so many ethnological studies, clearly to be counted evolutionary, appear in the years right after 1859 that Darwin's book appears to have loosed an avalanche. It may be instructive to recall that biological transformism was a particular expression of the evolutionary ferment, neither leading the way nor standing alone; and when it took the authoritative form of *The Origin*, it filled out one side of the evolutionary program. Biological transformism was a part of that program in its own right at the same time that it confirmed a larger set of ideas, but Darwinism alone did not cause the outpouring of cultural anthropology in the evolutionary mode.

Neither Tylor, Morgan, nor Lubbock had necessary recourse to "nature red in tooth and claw." Theirs was the evolution of development and continuity, not that of the newer vogue of evolution through ordeal. Without negating Darwinism, Tylor explicitly divorced the thesis of *Primitive Culture* from the Darwinian-Spencerian phrasing of the evolutionary process, but he saw the necessity of clarifying the relation when he affirmed the gradual growth of culture. Morgan, embracing the idea, was leery of the word "evolution." Tylor preferred the term "development." Only Lubbock pointedly drew Darwin and Spencer into his discussion of cultural evolution. He knew them both as friends and was an early partisan of Darwin's views; nonetheless, their names were primarily useful to Lubbock in a concluding chapter when he marshalled the witnesses for progress. Although the principle of continuous development, as the cultural anthropologists used it, presupposed Darwin's primeval man and opposed creationism, it was no rigid doctrine and depended upon no special and formal theory of evolution. Still, the ethnologists profited by the Darwinian climate of opinion, which was congenial to their work. And the biologist's findings, carrying the imperative stamp of scientific authenticity, supplied a strategic support to the whole enterprise of an evolutionary ethnology.

The anthropology of Tylor, Lubbock, and Morgan represents a convergence of ideas which had successively enveloped geology, biology, , and archaeology. The interchange among these sciences was like a cross-fertilization, and discoveries and theories in one field were not easily contained within narrow limits. By the time the evolutionary idea in its nineteenth-century dress was firmly entrenched in them all, geologists and biologists presented a common front with paleontologists, archaeologists, and students of culture. A composite of intertwining effects, their formalized views had almost the status of a scientific "ideology" to which they were all committed.

If the ethnologists were not eager Darwinians, their intellectual debts to the community of science is not thereby lessened. The dependence of Darwin's and Wallace's work on Lyell's geology is clear. Thereby nature's new chronology outran the five thousand years allotted to history by the theologians. With the realization of man's ancient age, the antiquarian's interest in pre-Roman curiosities was converted into the archaeological study of pre-history. But early man's antiquity and even his humanity are guaranteed by the fossil record assessed by geologists and paleontologists. Although the evolutionary ethnologists neither explicitly adopted nor rejected Darwin's biology, a theory which pointed to the descent of *Homo sapiens* from an earlier progenitor and to the common animal ancestry of the various sub-species of man was the premise which most suited their ethnological systems. But this statement is sketchy even for a summary, and a more detailed account of the background that produced the evolutionary movement in anthropology is in order.

2. THE EMERGENCE OF THE EVOLUTIONARY IDEOLOGY

It was difficult to accept the thesis of *The Origin of Species* without denying the Mosaic chronology and without recognizing that the gradual changes assumed in the mutability of species were also assumed in Lyell's geology. It was just as difficult to reject biological evolution without also rejecting both civilization as it was conceived by the evolutionary anthropologists and the antiquity of man. When the uniformitarianism of the *Principles of Geology* is admitted, that species evolve, that man is a primate, and that culture is a gradual, communal accumulation cease to be absurd propositions.[1] Those who opposed these views were aware of this cluster of mutually supporting ideas and combated it with another. In *The Recent Origin of Man* (1875), for instance, James C. Southall held that man is a recent creation (denying Darwin) who began life in a civilized state (denying Tylor, Lubbock, and Morgan) surrounded by a world of deluge and cataclysm (denying Lyell). Southall reminds us that the common allegiance of the men who were identified with the biology, geology, paleontology, and ethnology of the period bears out a specific scientific ideology. He grouped Lyell, Lubbock, Darwin, Huxley, Falconer, Prestwich, Geikie, Evans, Busk, and others together as those who "believe in 'natural selection' or 'evolution,' and all of them laugh to scorn the idea that Man is not more than seven or eight thousand years old."[2] Wallace and Agassiz broke the common front, both of them maintaining the diversity of origin of species, but almost everyone recognizes the anomaly of their position. Certainly Darwin did. Writing to Wallace in his own mild way, Darwin argued that, having gone so far, Wallace had abruptly stopped short and made a special case for human creation, completely distinct from the evolution of other species: "As you expected, I differ grievously from you, and I am very sorry for it. I can see no necessity for calling in an additional and proximate cause in regard to man."[3] And in a letter to Lyell, Darwin quoted a letter from Asa Gray about Agassiz and added, "Is not this marvelous?"

> . . . [Agassiz] coming near to deny that we are genetically descended from our great-great-grandfathers; and insisting that 'evidently affiliated languages, e.g. Latin, Greek, Sanscrit, owe none of their similarities to a community of origin, are all autochthonal'; Agassiz admits that the derivation of languages, and that of species or forms, stand on the same foundation, and that he must allow the latter if he allows the former, which I tell him is perfectly logical.[4]

[1] Huxley writes in the chapter he contributed to Darwin's *Life and letters:* "I have recently read afresh the first edition of the 'Principles of Geology'; and when I consider that this remarkable book had been nearly thirty years in everybody's hands, and that it brings home to any reader of ordinary intelligence a great principle and a great fact—the principle, that the past must be explained by the present, unless good cause be shown to the contrary; and the fact, that, so far as our knowledge of the past history of life on our globe goes, no such cause can be shown—I cannot but believe that Lyell, for others, as for myself, was the chief agent for smoothing the road for Darwin. For consistent uniformitarianism postulates evolution as much in the organic as in the inorganic world. The origin of a new species by other than ordinary agents would be a vastly greater 'catastrophe' than any of those which Lyell successfully eliminated from sober geological speculation." Darwin, Francis, *The life and letters of Charles Darwin* **1**: 543-544. New York, D. Appleton, 1887. Huxley did not need *The origin of species* to reveal the connection between the principles of geology and biology. Several months before its publication, in a lecture titled "Time and Life" before the Royal Institution of Great Britain, Huxley had maintained species development from parent stocks on a geological analogy, stating: "The doctrine of uniformity and the doctrine of progression are, therefore, perfectly consistent; perhaps, indeed, they might be shown to be necessarily connected with one another."

[2] Southall, James C., *The recent origin of man*, 45, Philadelphia, J. P. Lippincott, 1875.

[3] Darwin, *Life and letters* **2**: 297.

[4] *Ibid.*, 152-153.

When Darwin went aboard the *Beagle* as naturalist, he carried with him the first volume of Lyell's *Principles of Geology*. His old botany teacher at Cambridge, J. S. Henslow, pressed the book on him and then warned that he was not to believe what Lyell had written. Darwin found there an exposition of Lamarck's evolutionary biology as well as a method of interpreting geological data that made successive modifications according to regular and uninterrupted laws sufficient to account for geological phenomena.[5] The fluvialist and catastrophist geology of Cuvier was not only dispensable as a scientific principle; it was an unnecessary myth. With enough time and Lyell's idea of change as gradual modification, no mountainous waves of water or upheavals of rock were needed to explain the formation of valleys and rock deposits, and it was just this concept of change and an immense time span that Darwin needed for *The Origin of Species*. Darwin wrote in his *Autobiography*:

> After my return to England [from the voyage of the *Beagle*] it appeared to me that by following the example of Lyell in Geology, and by collecting all facts which bore in any way on the variation of animals and plants under domestication and nature, some light might perhaps be thrown on the whole subject.[6]

Darwin was the first to recognize that his theory depended on geological time. In 1856 he wrote to Hooker, "This power of [natural] selection stands in the most direct relation to time, and in the state of nature can be only excessively slow,"[7] and later, in *The Descent of Man*, Darwin made the antiquity of man "the indispensable basis for understanding his origin."[8]

Darwin dedicated his *Journal of Researches* to Lyell in 1845, and said thirty years later, "I never forget that almost everything which I have done in science I owe to the study of his great works."[9] Darwin's indebtedness to Lyell, however, is nowhere more apparent than in *The Origin of Species* itself:

> I am well aware that this doctrine of natural selection . . . is open to the same objections which were at first urged against Sir Charles Lyell's noble views on 'the modern changes of the earth, as illustrative of geology'; but we now seldom hear the agencies, still at work, spoken of as trifling and insignificant, when applied to the excavation of the deepest valleys or to the formation of long lines of inland cliffs. Natural selection acts only by the preservation and accumulation of small inherited modifications, each profitable to the preserved being; and as modern geology has almost banished such views as the excavation of a great valley by a single diluvial wave, so will natural selection, if it be a true principle, banish the belief of the continued creation of new organic beings, or of any great and sudden modification of their structure.[10]

There can be no doubt that the only explanation-of-species change that Darwin allowed was the kind that even cattle breeders and pigeon fanciers knew about. Uniformitarianism, geological time, and evolution were transposed intact to Darwinian biology. The ethnology of Tylor, Lubbock, and Morgan was based on the same principles and ran counter to the same opposition, for evolutionary anthropology was an outgrowth of and part of this development in geology and biology.

However much Darwin was making evolution basic to the derivation of all other species, he was reluctant in *The Origin of Species* to go beyond saying that "by this work light will be thrown on the origin of man and his history." A great deal has been made of the fact that this is the only explicit reference to man in *The Origin* and that it is almost an aside. Darwin included the remark, he said, in order not to conceal his own views on the origin of human species. For the great majority of readers Darwin, postulating the gradual development of animal species, need have said nothing to have implicated *Homo sapiens* in the evolutionary process too. Darwin's reasoning must have been close to the thinking of all those who considered the question of species derivation. "As soon as I had become . . . convinced that species were mutable productions, I could not avoid the belief that man must come under the same law."[11] Lyell saw this at once. His uncertainty and wavering, illustrated by his remark that Agassiz's theory of progressive development was "'so delightful, that he could not help all the time wishing that it was true,'"[12] came precisely from

[5] The principle of uniformitarianism, usually associated with the English geologists Hutton and Lyell, is clearly enunciated in Lamarck's *Philosophie zoologique*, 1809.

[6] Darwin, *Life and letters* **1**: 67–68.

[7] *Ibid.*, 445.

[8] Darwin, Charles, *The descent of man*, 2, New York, D. Appleton, 1886.

[9] Darwin, *Life and letters* **2**: 374.

[10] Darwin, Charles, *The origin of species*, 98, New York, D. Appleton, 1872.

[11] Darwin, *Life and letters* **1**: 75–76. Specifically and more prosaically, in Darwin's words: "*Our* ancestor was an animal which breathed water, had a swim bladder, a great swiming tail, an imperfect skull, and undoubtedly was a hermaphrodite!" *Ibid.*, **2**: 60.

[12] *Ibid.*, **1**: 403.

realizing the implication of *The Origin of Species* for man:

> It is this which has made me so long hesitate, always feeling that the case of Man and his races, and of other animals, and that of plants is one and the same, and that if a "vera causa" be admitted for one, instead of a purely unknown and imaginary one, such as the word "Creation," all the consequences must follow.[14]

The coming of *The Origin* was so dramatic and its impact created such a stir that "evolution" is now inseparable from Darwin's name. For many they are one and the same word. Certainly, Darwin's prominence dwarfs that of his predecessors, and college undergraduates are convinced, despite what their instructors tell them, "No Darwin, no evolution." Darwin did not foster this extravagant claim. He made a great point of coupling his own with Wallace's name when referring to the originators of "our theory." To insure that the scientific world should not miss what he had to say Darwin put gift copies of his book in the hands of prominent professionals at home and abroad. An aroused public, fearing the worst and whetted by the intervention of Hooker and Lyell in the stir over authorship with Wallace, exhausted a first printing of 1,250 copies in a day.

For a scientific text, the book found a surprising sale among clerics; and it would be hard to say whether biologists or bishops felt more was at stake. Darwin was to have friends in both ranks, but not in equal numbers. The clash of notables in public debate, with no less than Huxley and Wilberforce as dramatis personae, gave the whole event a genuine element of theater. The two camps met head on in the British Association meeting of 1860. The principals themselves caricature the great antagonism —Bishop Wilberforce confident, popular, and proud, dressed in the garb of high office, with a keen mind slick with rhetoric and stubborn with dogmatism, so bound to be right that he said more than he knew, and Huxley from a younger generation, technically trained in anatomy and physiology, smarting at the bishop's presumptuousness and stung by this spokesman for the vested interests of science and the church, retorting that it would have been better if Wilberforce had read Darwin's book.

Overshadowing his predecessors, Darwin generously and rightfully added "An Historical Sketch" to the sixth edition of *The Origin*, recalling such names as Lamarck, Saint-Hilaire, Goethe, and his grandfather Erasmus. Darwin's list grew until he named thirty-four men who believed "in the modification of species, or at least disbelieved in separate acts of creation." Many, with far less claim than Wallace, were willing to be counted the originators of "natural selection," including the anatomist Richard Owen, who was on public record against it. One cantankerous gentleman, Patrick Matthew, had name cards printed bearing the additional identification "Discoverer of the Principle of Natural Selection." Herbert Spencer never did quite forgive his contemporaries for overlooking his paternity of the new evolutionary movement. He was particularly chagrined because, having made the discovery, he felt that he had done so much more with it than Darwin.

Perhaps the point is that it took Darwin's whole interlocked theory displayed by a world of facts, all patiently interwoven like a massive exercise in inductive and deductive logic, to make those claims to fame worth making. Although the evolutionary idea was not a new one, its pedigree became significant in the light of Darwin's achievement. Huxley said it as well as anyone in the preface to *Darwiniana* when he wrote of "the ancient doctrine of Evolution, rehabilitated and placed upon a sound scientific foundation, since and in consequence of, the publication of the '*Origin of Species*'. . . ." Darwin canonized, St. Darwin the Major, reflected glory on many a minor evolutionary saint.

In the same way, after de Perthes' vindication, half-forgotten or ignored archaeological discoveries assumed new importance. They had been nothing but oddities under the old dispensation of Cuvier and signified little or nothing about primitive man and his technology. They attracted little attention, for they were considered trivial or at most interesting; and the record of their discovery comes to light only slowly and piecemeal. It turns out that chipped tools were being discovered around Rome in 1846; and in 1850 river-drift implements were found in the lower Apennines near Imola. Similar findings were made in Toulouse in 1851. In addition there is the history of cave researches in England, Belgium, and Germany going back as far as 1800. Still earlier, in 1797, John Frere gave his "Account of Flint Weapons Discovered at Hoxne in Suffolk" to the Society of Antiquaries of London and described stone weapons that were unearthed twelve feet below the surface in stratified

[14] *Ibid.*, **2**: 2.

deposits. But the oldest find of them all was the flint tool which was turned up at Gray's Inn Lane in London, about 1690, near the remains of an elephant-like beast. After de Perthes' claims were verified these discoveries ceased to be idle and miscellaneous facts.

What prevented the evaluation of these findings as the revolutionary facts that they were? Time and again a bold antiquarian did so, only to be ridiculed by the savants who spoke in the name of authority. The climate of opinion was on their side, and that was enough. A ready-made interpretation consistent with the regnant geology was close at hand. Everything was accountable in one way or another—as a freak of nature, as the result of violent upheaval and flood, as the work of elves, or even as tests to try men's faith. Facts took their place under the accepted premises, and the prevailing beliefs were maintained. In 1690 nobody paid much attention when a crudely shaped tool was uncovered near the skeleton of an "elephant" in London, but there was little reason to. When the same discovery is reported in the vocabulary of modern science, however, a new import emerges: "The first paleolithic flint implement ever recognized and preserved as a relic of ancient man was a Chellean tool of the cleaver type discovered in Pleistocene gravels at Gray's Inn Lane, London." Expressed in this way, the "facts" are radically different.

They are startling facts only when they are interpreted with the insight appropriate to modern times. As recent a geologist as Dean Buckland, Lyell's teacher at Oxford, steadfastly refused to grant that human bones and chipped flint tools were older than the "flood," even when they were found in England next to rhinoceros remains. Buckland himself had explored Goat's Hole Cave in 1823 and had found the "Red Lady of Paviland," but even that field work did not move him to think in temporal terms before the Romano-British period. Archaeological and paleontological discoveries waited upon the new geology to locate their place in time. Primitive human remains, even if the evidence was scanty, found a precise place in the mutable primate order prepared by Darwin. It did not take Boucher de Perthes' roughly hewn tools to give early man a mutable culture, but they did give a glimpse of how far man had come in the technical arts. A whole new set of ideas thus accumulated to put the meaning of antiquarian discoveries in a radically different perspective. Heretofore, belief could rest comfortably in the old, familiar terms; but now there was not any choice, and belief was compelled to accommodate de Perthes, Darwin, and Lyell.

3. THE EVOLUTION OF CULTURE IN THE CONTINUOUS COMMUNITY

Coupled with the discoveries of Boucher de Perthes in the Somme Valley, the consequences of *The Origin of Species* bore directly on the new anthropology. The flint tools in the river gravels, authenticated at the same time that *The Origin* was published, dramatically posed the specific issue of a naturalistic derivation of culture while the biology of Darwin symbolized the animal origin of man and the rejection of creationist theories of his appearance in the world. Although the flint axes of man's primitive industry were more immediately germane to the ethnology of Tylor, Lubbock, and Morgan than to Darwin's biology, both the nature of man and the method by which his animal pedigree was established were logically central to their ethnology. The "zero point," as Morgan called it, from which cultural evolution began was delineated by the crude industrial remains found by Boucher de Perthes. But this zero point gained clarity and significance when correlated with the existence of Tertiary mammoth and when Huxley's analysis of the Neanderthal skull disclosed a cranium with pithecoid traits and anthropoid brain capacity.[14] The age of Neanderthal man was undetermined, and Huxley did not argue that the primordial ancestor of modern man had been found; but the close resemblance between the "ancient skulls and their modern Australian analogues" immediately suggested to him that between the culture of modern and ancient savages there was possibly another analogy. Primeval man, whoever he was, was a lineal descendent with the pithecoids from a common primate stock, and a primitive flint technology seemed a more appropriate handiwork for early man than the civilization claimed for him by Archbishop Whately. Whately reasoned that the early condition of man was such that progress was impossible without divine assistance;

[14] Fittingly enough, material which Huxley used to locate man's place in nature found a place in Lyell's geological treatise on human antiquity and Darwin's biological treatment of the origin of man. Darwin quotes in full Huxley's "Note on the Resemblances and Differences in the Structure and the Development of the Brain in Man and Apes" in *The descent of man*, and Huxley's analysis of the Engis and Neanderthal skulls is appended to a chapter of Lyell's *Evidences of the antiquity of man*.

and although uncivilized man had the capacity to learn from a community which had already advanced beyond a state of savagery, there was still the necessity of launching this special group on the path to civilization. Whately wrote:

> According to the present course of nature, the first introducer of cultivation among savages, is, and must be, Man, in a more improved state: in the beginning therefore of the human race, this, since there was no *man* to effect it, must have been the work of *another Being*. There must have been, in short, a *Revelation* made, to the first, or to some subsequent generation, of our species. And this miracle (for such it is, as being an impossibility according to the present course of nature) is attested, independently of the authority of the Scripture-accounts, by the fact, that civilized Man exists at the present day.[15]

One miracle called for another, and Archbishop Whately's pre-Darwinian frame of mind made him look for distinct breaks and origins where there was only continuity. It was man created, distinct and entire, whose culture was necessarily created also. On the other hand, it was primate man, who shared a common lineage with Neanderthal kin, whose cultural "zero point" was fixed in the first rude fashioning of stone implements.

It is the contribution of the ethnologists of this period that, by using the evolutionary method, they divorced culture or civilization from the predestined order of a divine provision and, by making culture the works of man, they found its origin in the continuous community. Although they insisted that cultural evolution was on the whole progressive, their problem was not, assuming a natural order of progress, to investigate the interferences and unnatural obstacles which prevented perpetual advancement.[16] They were fundamentally insisting that culture had evolved and had not been "created" and, further, that it was reasonable to assume that man might have reached nineteenth-century civilization by slowly adding on to "what lies at hand," particularly if social evolution extended to the remote time of Neanderthal man's ancestors. Without exception, Tylor, Lubbock, and Morgan thought well of the culture in which they lived and measured progress in terms of monogamous marriage and the industrial revolution, so that steps in the direction of life as they lived it were termed "progressive." Nevertheless, the theme which engrossed them was the development of culture, and as much as anything else, it was the theme of evolution that committed them to a belief in progress. According to Tylor, the

> . . . standard of reckoning progress and decline is not that of ideal good and evil, but of movement along a measured line from grade to grade of actual savagery, barbarism, and civilization. The thesis which I venture to sustain, within limits, is simply this, that the savage state in some measure represents an early condition of mankind, out of which the higher culture has gradually been developed or evolved, by processes still in regular operation as of old, the result showing that, on the whole, progress has far prevailed over relapse.[17]

Asserted in the face of degeneration, the cultural evolutionists' theory of progress was a restatement of their essential naturalism, that human advancement was a "process still in operation as of old." Progress, in the words of the subtitle of Morgan's *Ancient Society*, was "from savagery through barbarism to civilization," and tampering with this order of development made it a completely unintelligible sequence. The idea of a primeval Golden Age, appearing in savagery, followed by savagery, wrecked the natural continuity of events and destroyed the foundation of a science of culture. Such an age of primeval glory, unattached and isolated, with savage culture before and after it, was inexplicable by evidence which the evolutionists recognized as valid, and not to deny its existence would have been fatal to cultural evolution.

The urgency of combating this myth was undoubtedly responsible for Lubbock's insistence that civilization is largely an indigenous development. Lubbock stressed this view more than Tylor and Morgan did and held to it in its most extreme form; it was he who challenged Archbishop Whately's statement that ". . . we have no reason to believe that any community ever did, or ever can, emerge, unassisted by external helps, from a state of utter barbarism into any thing that can be called civilization. . . ."[18] Lubbock carried on the controversy with the Duke of Argyll, who shared Whately's point of view,

[15] Whately, Richard, *Political economy*, 120, London, B. Fellowes, 1832.

[16] This is precisely Malthus' problem in *An essay on the principle of population* (1803), illustrated by his beginning, ". . . the mode of conducting the subject which naturally presents itself, is: 1. To investigate the causes that have hitherto impeded the progress of mankind toward happiness; and 2. To examine the probability of the total or partial removal of these causes in the future." Malthus, T. R., *An essay on the principle of population*, 1, London, n. p., 1803.

[17] Tylor, Edward B., *Primitive culture* 1: 2, New York, Henry Holt, 1883.

[18] Whately, *Political economy*, 106–107.

and reprinted his attacks on the degeneration theory of Whately and the Duke in an appendix to *The Origin of Civilisation*. In the last chapter of this book Lubbock concluded: "That existing savages are not the descendants of civilised ancestors. That the primitive condition of man was one of utter barbarism. That from this condition various races have independently raised themselves." [19] To deny Whately's contention that savages could not raise themselves without outside help, either from a divine source or from a civilized race which had itself received divine assistance, Lubbock maintained that savages even as savages could independently evolve a civilized culture. He thereby underestimated the importance of cultural diffusion as a factor in cultural change, an oversight which has embarrassed ever since those sympathetic to the cultural evolutionists' position. In the absence of diffusion to account for the similarity of cultural development, Lubbock fell back on the psychic unity of mankind.

4. JOHN LUBBOCK: THE REVOLUTION AS A SYNTHESIS

The figure of John Lubbock is particularly instructive as an ideal type illustrating the new movements of thought in England in the crucial period from 1830 to 1870. More than either Tylor or Morgan, he personified the way in which Lyell's geology, Darwin's biology, and the discoveries of Boucher de Perthes converged to generate the new anthropology. In the preface to *The Origin of Civilisation* (1870), Lubbock dated the beginning of his ethnological studies from 1860, and Tylor said in the preface to *Primitive Culture* (1871) that his researches covered a "course of ten years," putting the beginning of their systematic interest in anthropology within a year or two after Darwin had published *The Origin of Species* and Boucher de Perthes had been vindicated. For Lubbock the connection was direct and specific because he was personally identified with the new biology and the recognition of the genuineness of Boucher de Perthes' findings, and Lubbock's career as geologist, zoologist, botanist, and archaeologist brought together the material and method out of which modern anthropology emerged. When Lubbock was raised to the peerage after thirty years in the House of Commons and after a distinguished and many-sided career in science, he was given the title of Lord Avebury. The name came from the megalithic temple of Avebury, which he and Tyndall had investigated while determining the relative antiquity of Silbury Hill and the Roman road which passed by it. When Lubbock later bought the stone circle at Avebury in order to save it from use as a building site, the ethnologist claimed his primitive heritage, and Sir John Lubbock gained a property right to the title of Avebury.

One of the earliest and most enthusiastic converts to Darwinian biology, Lubbock based his subsequent archaeological and ethnological researches on the conception of "man's place in nature" with which the new biology was associated. Along with Tylor, he inaugurated in 1865 the evolutionary period in anthropology in England, for in that year appeared Lubbock's *Pre-Historic Times* as well as Tylor's *Researches into the Early History of Mankind*. In both these ethnological classics culture was treated historically. The development of civilization from its barbaric origins was their central theme, and this was the theme which dominated anthropology for almost half a century. The struggle over biological evolution paralleled the period in which evolutionary anthropology and sociology dominated the study of social man, and the full tide of cultural evolution seemed almost to wait on *The Origin of Species* as its catalyst. Although historians of anthropology have never agreed on the precise role of Darwin in the development of social thought in the nineteenth century, all of them recognize the common temper of *The Origin of Species* and such anthropological works as Lubbock's *The Origin of Civilisation* and Tylor's *Researches*, subtitled *The Development of Civilisation*.

John Lubbock was one of the men whom Darwin grouped with Lyell, Hooker, and Huxley as competent critics of *The Origin of Species*. Darwin sent Lubbock a copy of his book before it was released to the public and wrote him, "I care not for Reviews; but for the opinion of men like you. . . ." [20] At the meeting of the British Association in 1860, Lubbock stood with Hooker and Huxley and "spoke up for Natural Selection in the great Oxford debate" [21] by contributing a paper on the embryological evidence for evolution. Lubbock was then twenty-six, and his re-

[19] Lubbock, John, *The origin of civilisation*, 479–480, New York, D. Appleton, 1882.

[20] Darwin, *Life and letters* 2: 15.

[21] Lubbock, John, *Essays and addresses*, 1–2, London, Macmillan, 1903.

lation to Darwin went back to his boyhood. The Darwins moved to Down when John Lubbock was eight, and three miles east of Down was High Elms, the Lubbock residence. Darwin and Lubbock's father—an astronomer, banker, and mathematician—were friends, and both were members of the Royal Society. As John Lubbock grew older he saw more and more of Darwin, assisting him with experiments and writing his first published scientific paper on some of Darwin's collections. When the local Vicar sermonized on "the infidel and naturalist in our midst" with Darwin and Lubbock both present, Lubbock immediately identified the naturalist as Darwin and "hoped that he might share the partnership by representing the other." [22]

Already an ardent Darwinian, in 1860 Lubbock joined the trek of English scientists across the channel to see firsthand the Somme quarries in which de Perthes had been finding flint implements since 1838. The *Natural History Review*, edited by Lubbock and Huxley, published, in the July, 1862, issue, Lubbock's estimate of the authenticity of de Perthes' claim to the discovery of what he called *haches antediluviennes* in the river gravels around Abbeville. The weight of Lubbock's evidence was geological, as is evident from the title, "On the Evidence of the Antiquity of Man, Afforded by the Structure of the Somme Valley," and Lubbock pressed home his argument of the antiquity of man by postulating the equal antiquity of the Somme Valley. This essay anticipated Lyell's comprehensive treatment in *Geological Evidences of the Antiquity of Man* of the next year and became part of the text of Lubbock's ethnological study *Pre-Historic Times*. The eons which Lyell's *Principles of Geology* had allowed Darwin to assume in the evolution of species became a necessary premise for ethnologists in the evolution of culture.[23] The basic problem of the anthropologists of this period had taken shape. Man was not only a primate, he was a tool-making primate, and his technology was evolved no less than his anatomy from a remote anterior origin. Archaeological discoveries graded chronologically by geological techniques were making it increasingly difficult to ignore the long history of man, especially when *The Origin of Species* pointed to the same antiquity. Lubbock closed his essay:

> From the tombs at St. Acheul, from the Roman remains found in the superficial layers of the peat, at about the present level of the river, we know that fifteen hundred years have produced scarcely any change in the configuration of the valley. In the peat, and at a depth of about fifteen feet in the alluvium of Abbeville, are the remains of the neolithic period, which we have ample reason for believing, from the researches in Denmark, Switzerland, and other countries, to be of no slight antiquity. Yet all these are subsequent to the excavation of the valley. What date then are we to assign to the men who lived when the Somme was just beginning its great task? [24]

What Lubbock saw in the Amiens and Abbeville deposits and in the collections of Boucher de Perthes did not shock him. Before going to France Lubbock had served on a committee of the British Association with Lyell, Busk, Evans, Pengelly, and others, under whose supervision Kent's Hole, near Torquay, had been examined, and the evidence there pointed to the simultaneity of man and mammoth. The excavation in Kent's Hole went back to 1825, and between 1825 and 1859 two competent investigators had sent memoirs to the Geological Society attesting the antiquity of man. The first report, by Godwin-Austin, was ignored; the second, by Edward Vivian, was considered so incredible that it was never published at all. Before either of these reports was made, MacEnery, a Catholic priest in the neighborhood of Torquay, had found flint tools with the bones of the mammoth and rhinoceros in loam covered with stalagmite. But Buckland, the first Reader of Geology at Oxford and later Dean of Westminster, had already declared in *Reliquiae Diluvianae* (1823) "that none of the human bones or stone implements met with . . . in any of the caverns [that he had seen] could be considered to be as old as the mammoth and other extinct quadrupeds," and, when he and MacEnery corresponded about the evidence of Kent's Cavern, Buckland reached the same conclusion.[25] He accounted for the implements found

[22] Duff, Adrian Grant, *The life-work of Lord Avebury*, 15, London, Watts, 1934.

[23] Tylor explicitly recognizes this when he says that the ". . . chronological demand [of the development theory of culture] must be no small one, and the more so when it is admitted that in the lower ranges of culture progress would be extremely slow in comparison with that which experience shows among nations already far advanced." Tylor, *Primitive culture* 1: 55.

[24] Lubbock, John, *Pre-historic times*, 359, New York, D. Appleton, 1908.

[25] Lyell, Charles, *The antiquity of man*, 97, Philadelphia, George W. Childs, 1863. The subtitle of *Reliquiae diluvianae* was *Observations on the organic remains contained in caves, fissures, and diluvial gravel and on other geological phenomena attesting the action of an universal deluge*. In 1836 Buckland published *Geology and mineralogy considered in relation to natural theology*.

by MacEnery by supposing that "Ancient Britons" had dug holes in the stalagmite for ovens and that the tools had passed through these holes, thus intermingling with the bones of extinct animals. Out of deference to the authority of Buckland, MacEnery did not publish his conclusions, and they remained in manuscript until 1859, when an abstract was published by Vivian.

On the continent Cuvier, declaring categorically that "l'homme fossile n'existe pas," performed a similar service for de Christol, Tournal, and Schmerling, who were discovering human bones and sometimes crude pottery intermixed with mammoth remains.[26] The general presupposition was that they were washed together by a diluvial catastrophe, of the kind that abounded in Cuvier's and Buckland's geology. Boucher de Perthes, meeting with the same incredulity, said in desperation, "It is the workmen who help me, not the geologists."[27] Lubbock was fully aware of the prejudice against admitting the Pleistocene age of man, and the bias which accounted for de Perthes' treatment "as an enthusiast, almost as a madman," Lubbock ironically called a tribute to the properly conservative temper of geological science.[28] The evidence of human antiquity, however, was piling up, and the geology of Cuvier and Buckland was already under attack.

Brixham Cave, also near Torquay, was opened in 1858, and the paleontologist Hugh Falconer wrote the Geological Society that it contained a stalagmite terrace in which bones were embedded and that he had seen them himself. Falconer suggested that "the principal deposit of fossil bones may be looked for under the stalagmite floor not yet touched" and that the cave was "deserving of a combined effort among Geologists to organize operations for having it satisfactorily explored, before mischief is done by untutored zeal and desultory work."[29] Falconer, Busk, Evans, Pengelly, Prestwich, and Ramsay were appointed to supervise the investigation, and Falconer prepared a preliminary report for the Royal Society in 1858, vouching for the contemporaneity of flint tools and *Rhinoceros tichorinus*. Because of his health Falconer had to leave England the same year and go to Southern Europe. On his way he stopped in Abbeville in order to see Boucher de Perthes' collection. From there he wrote the geologist Prestwich:

> . . . let me strongly recommend you to come to Abbeville. I am sure you would be richly rewarded. You are the only English geologist I know who would go into the subject *con amore*. I am satisfied that English geologists are much behind the indications of the materials now in existence relative to this walk of *Post-Glacial* geology, and you are the man to being up the lee-way.[30]

The following year, 1859, John Evans, of the Society of Antiquaries, and Prestwich investigated the French quarries. In his presidential address to the Geological Section of the British Association in 1859, Lyell supported Falconer's report on the Brixham Cave and corroborated Prestwich's memoir to the Royal Society on the proof of man's antiquity. The stream of British scientists to the Somme Valley in order to see firsthand the excavations around Abbeville and Amiens began.

Lubbock interpreted the events of 1859–1860 as an intellectual revolution and predicted that the furor over the controversy would soon pass. In the first edition of *Pre-Historic Times*, he wrote:

> The new views with reference to the Antiquity of Man, though still looked upon with distrust and misapprehension, will, I doubt not, in a few years be regarded with as little disquietude as are now those discoveries in astronomy and geology which at one time excited even greater opposition.[31]

Traditional usages and ideas, the relics of an earlier time persisting into the present were later to attract Lubbock's attention when he dealt with primitive social customs and technology. Under the impact of *The Origin of Species*, the geology of Lyell, and the work of the evolutionary anthropologists, the intellectual framework of the traditional concepts of man and history symbolized by Usher's chronology, Paley's *Natural Theology*, Buckland's *Reliquiae Diluvianae*, and the *Bridgewater Treatises* were themselves to become "survivals."

Lubbock's archaeological researches began in earnest. Sometimes alone—sometimes with Prestwich, Evans, and Busk—Lubbock scoured western Europe for ethnological materials. In three years he examined almost every gravel pit from Amiens to the sea, visited the principal

[26] de Mortillet, Gabriele et Adrian, *Le préhistorique origine et antiquité de l'homme*, 10, Paris, Schleicher Frères, 1900.

[27] Daniel, Glyn E., *A hundred years of archaeology*, 59, London, Gerald Duckworth, 1950.

[28] Lubbock, *Pre-historic times*, 320.

[29] Murchison, Charles, *Paleontological memoirs and notes of the late Hugh Falconer* 2: 486, London, Robert Hardwicke, 1868.

[30] *Ibid.*, 597.

[31] Lubbock, *Pre-historic times*, ix.

Kjökkenmöddings (shell-mounds) in Denmark, studied the Swiss Lake-dwellings, and haunted public museums and private archaeological collections. At the same time he learned enough Danish to read in the original the Danish archaeologists on Kjökkenmöddings. The *Natural History Review* contains the tangible products of these investigations in the issues of 1861 and 1862; these articles form the backbone of Lubbock's *Pre-Historic Times*. The gap between Kent's Hole and Lubbock's ethnology had been bridged.

Lubbock thought of archaeology as the link between geology and history, and what he meant by "history" was "ethnology." His career bears out this connection and adds another, the influence of biology, for as a young naturalist he was tutored by Darwin in botany and zoology. When Lubbock was twenty-one, Lyell sponsored his election to the Geological Society. Three years later he was elected to the Royal Society and had met the men of science responsible for the intellectual climate of mid-nineteenth-century England—Hooker, Kingsley, Prestwich, Evans, Tyndall, Busk, Huxley, and of course, Lyell and Darwin. Becoming the youngest member of the X Club, Lubbock joined a group of men which included so many famous personages of scientific England that gossips claimed that the club "ran science" and that presidents of the Royal Society were elected in its privy councils. It was a formidable group. In addition to Huxley, the members were "George Busk, secretary of the Linnean Society, Edward Frankland, President of the Chemical Society; T. A. Hirst, head of the Royal Naval College at Greenwich; Sir Joseph Hooker, and W. Spottiswoode, Presidents of the Royal Society; Herbert Spencer, and Tyndall." [32] They ate together just before the meetings of the Royal Society and three successive presidents of the Royal Society came from their group, giving credence to the gossip. Lubbock later became president of the British Association, the Ethnological Society, the Anthropological Institute, and the Society of Antiquaries almost as a matter of course.

He stood for Parliament in the same year that his *Pre-Historic Times* appeared, and Darwin thought it cost him the election. After Darwin read *Pre-Historic Times*, he wrote Lubbock, "I do sincerely wish you all success in your election and in politics; but after reading this last chapter, you must let me say: 'oh, dear! oh, dear! oh dear!' " [33]

When Lubbock entered politics, his scientific friends felt that they were losing a capable colleague and that Lubbock was giving up a distinguished career in science. They may have been right. Already a prominent naturalist and ethnologist, he was also a banker (Lubbock inherited the family bank and was active in its management), and he was soon to gain a seat in the House of Commons and to hold it for thirty years. The diversity of his interests spread his energy thin; his family was under the impression that he had an "annual meeting" every day. Lubbock epitomizes a great Victorian, but after fifty years, he is almost unknown. His place in the launching of a science of culture remains unique, primarily for the reason that its background is Lubbock's own.

In brief, the evolutionary movement in anthropology may be resolved into the general context of nineteenth-century science, and something has already been said about the origin of the new ethnology and the various intellectual currents with which its growth was associated. The ethnologists not only shared a background but an urgency, almost a sense of mission, which was also characteristic of the militant scientists allied with Darwin. The motive which gave Lubbock, Tylor, and Morgan this mission and made the "development theory of culture" a necessity for them is part and parcel of the aggressive naturalism of the period. The decorous and grammatical pronouncements of these Victorian spokesmen for science should not obscure their deep partisanship. They were engaged in a serious business. Their commitment to the evolutionary method stems from their prepossessing task, the creation of what Tylor called, without embarrassment, "The Science of Culture." They looked to the secular, mundane, and ordinary to relieve human behavior of causeless spontaneity and supernatural influence. Their excesses in speculation and logic, finding connections where there weren't any, had a basis in the conviction that the connections must be there if they could be found. Tylor maintained that the only proper antidote to "arbitrary impulses, causeless freaks, chance and nonsense and indefinite accountability" was "to see the line of connexion in events." [34] There could be a science of culture if

[32] Lubbock, *Essays and addresses*, 2.

[33] Darwin, *Life and letters* **2**: 220–221.

[34] Tylor, *Primitive culture* **1**: 19.

cultural facts were seen as following a rational order in evolutionary growth.

5. THE SCIENCE OF CULTURE: PROGRESS NATURALIZED

Contemporary climate of opinion in the social sciences makes it difficult to remember that the first who attempted to treat human behavior as the subject of a science had to contend with the argument that *fact* and *social* were incommensurable. At the same time that Comte was labeling "sociology" in the *Cours de philosophie positive* (1839–1842), he was making a monumental attempt to justify its inclusion among the sciences by denoting its position as the last member of a scientific series and by patterning it in the form of an empirical physics applied to social phenomena. John Stuart Mill, the most influential Comtist in England, followed the same tack in the sixth part of his *Logic* (1843). He was persuaded with Comte that a social science is possible if its methodology is molded on the techniques of the physical sciences. The first question with which Mill had to deal in considering "The Logic of the Moral [Social] Sciences" was "Are human actions subject to the laws of causality?" It will be recalled that in *The Study of Sociology* (1873) Spencer raised essentially the same question, and in 1871 Tylor prefaced his *Primitive Culture:*

> Our modern investigators in the sciences of inorganic nature are foremost to recognize both within and without their special fields of work, the unity of nature, the fixity of its laws, the definite sequence of cause and effect through which every fact depends on what has gone before it, and acts upon what is to come after it. . . . To many educated minds there seems something presumptuous and repulsive in the view that the history of mankind is part and parcel of the history of nature, that our thoughts, wills, and actions accord with laws as definite as those which govern the motion of waves, the combination of acids and bases, and the growth of plants and animals.[35]

The universal scope of physical laws was still a premise to be examined, at least if it was made truly universal and extended to social behavior. The science of sociology waited on the physical sciences to make *fact* and *social* congenial terms, and as the phrases "physical science" and "social science" show, the terminology we use still reflects the ancient belief that the social province lacks the concrete and substantial character of the physical world. The science of man was gingerly broached in the phraseology of cause-and-effect relations, and from the physicists, Helmholtz, Mayer, and Joule came the confident authority that those relations were indeed universal. Spencer's putting *First Principles*, a volume which outlines his mechanistic approach, at the head of the "Synthetic Philosophy" is the supreme tribute to physics of a philosophy that would be social and scientific.

If there had hung about the animal kingdom the aura of miraculous creation until 1859 and if the no-less-miraculous geology of catastrophe and flood had been dispelled only thirty years before, it should be no surprise that the concept of "culture" as the works of man was a significant advance for anthropology and sociology. Clearly defined in Tylor's anthropology and later developed independently in Spencer's sociology, the idea that culture is both man-made and a natural social growth challenged the degeneracy theory of savages and the traditional belief that, in the beginning, there was a state of civilization. Now it may seem a remote and trifling point that early anthropologists had to combat the theory that primeval man began his social life with a rudimentary culture and that savages had seen better days; in reality, the conflict cloaked a far more significant issue. Franz Boas suggested that the first systematic comparative anthropology was the work of Christian fathers trying to understand the relation of paganism to Christianity and that they interpreted pagan heterodoxy as a falling away from an original orthodoxy. The notion of the degeneration of savages from an earlier state of comparative civilization, a position for which Archbishop Whately and the Duke of Argyll were outstanding spokesmen, retains the theological flavor of the Biblical fall of man and the origin of culture through divine revelation. Fully aware of the motivation behind the degeneracy theory and of its threat to a scientific cultural anthropology, Tylor pointed out that the grounds on which this position had been held "have generally been rather theological than ethnological."[36] The idea that culture is a human product and the idea that culture is a social heritage are supplementary, and a naturalistic concept of civilization hangs on the two together. That a man isolated from society might create civilization is fantastic. It would be a godlike act. Except on the assumption of continuity with a preceding community, it is equally fantastic

[35] *Ibid.*, 2.

[36] *Ibid.*, 36.

that even a community of men might create culture. Only if culture is a gradual development concurrent with the social history of the species, does civilization cease to be a miraculous creation. If culture is that which is "acquired by man as a member of society," as Tylor insisted, the antiquity of man and cultural evolution are necessary props to make this view of culture tenable.

For Tylor, Lubbock, and Morgan the community's social heritage was fundamental to a theory of progress and indispensable to a scientific theory of culture. Tylor quoted Humboldt's statement that "Man ever connects on from what lies at hand" almost as a text for scientific anthropology and found the principle of cultural continuity implied by the universal sequence of cause and effect. Morgan justified his contention that human progress increased in a geometrical ratio on the same continuity: "Every item of absolute knowledge gained became a factor in further acquisitions, until the present complexity of knowledge was attained." If culture was gradually developed in a process governed by unchanging laws, then cultural facts were no different from geological and biological facts, and all of them were the consequences of a uniformly behaving nature. Furthermore, on the assumption of the unity of nature, civilization, geological formations, and animal species developed from earlier civilization, earlier geological formations, and earlier animal species. The concept of culture as a cumulative social legacy was the central idea upon which the new anthropology depended. The geology of Hutton and Lyell, the biology of Darwin and Wallace, and the ethnology of Tylor, Lubbock, and Morgan formed a universe of discourse around evolution and the unity of nature. The principles and methodology which they came to share and the cross-checking and confirmation which each afforded the other is an instructive example of the common area of the sciences in the period 1830–1880.

It is often overlooked that in addition to their doctrine of progress it was the evolutionary method and the urgency of their using it against the creationists that landed Lubbock, Tylor, and Morgan in the "ethnocentric" predicament. They were persuaded that cultural evolution was not meaningless change and that a pattern, which gave "lines of development" significance, emerged in evolution. It was not so much that they failed to appreciate cultural context but rather that they were convinced that of primary importance was the development in that context which led to another stage of civilization. The most judicious member of the evolutionary school, Tylor, instead of denying that his definition of progress was determined by the particular circumstances of time and place, wrote:

> The educated world of Europe and America practically settles a standard by simply placing its own nations at one end of the social series and savage tribes at the other, arranging the rest of mankind between these limits according as they correspond more closely to savage or to cultured life.[37]

It is instructive that, when Tylor challenged the degradation theory of civilization in an earlier work, he specified that by the forward direction of progress he meant "the appearance and growth of new arts and new knowledge, whether of a profitable or hurtful nature, developed at home or imported from abroad."[38] From where else but western civilization might Tylor have taken this standard for measuring progress, if what he meant by "progress" was "modern," the newest development in the long continuity of culture?

Later, when the trend was away from historical anthropology and toward functional cultural studies, the emphasis shifted. Concentration on evolutionary lines of development gave way to examination of integrated cultural patterns, and "progress," instead of furnishing the rule by which one cultural unit is graded relative to another, became merely a bias, a facet of social context. For the Boas school in anthropology, and for Malinowski, Radcliffe-Brown, and others, the presumption of Tylor, Lubbock, and Morgan in passing judgments on cultures, taking their own civilizations as a yardstick, appeared astonishingly naive and provincial.

The evolutionists assumed what was to them the obvious and necessary starting point for their inquiry into cultural origins, and if they were to measure cultural development and degeneration, they required a criterion of progress. The controversy in which the evolutionists were engaged virtually dictated both the point from which their study began and the criterion of progress. Although cultural evolution and cultural progress are two different concepts, the evolutionary school treated them together. The chapter in which Tylor laid down a measure of progress in *Primitive Culture* is the same chapter in which he considered the evolution *versus* the creation theory of cultural origin; progress and evolution

[37] *Ibid.*, 26.

[38] Tylor, *Researches*, 181.

were both brought under "The Development of Culture." When Morgan considered the "Ratio of Human Progress" in *Ancient Society*, he recapitulated the transition from savagery to civilization. "By re-ascending along the several lines of human progress toward the primitive ages of man's existence, and removing one by one his principal institutions, inventions, and discoveries, in the order in which they have appeared, the advance made in each period will be realized."[39] Facing two issues, Tylor and Morgan found their stand on one dictating their stand on the other. At the same time that they were asserting the development theory of culture, they felt obligated to deny that primitive culture was a degeneration, and the way that they said both at once was to say that modern culture was a progressive development from barbaric culture—not that savages had fallen from civilization, but rather that the culture of the nineteenth century had progressed from rude beginnings. "Fuegian" culture or the civilization of the Australian aborigines was not so much a mystery or challenge to the development theory as was the civilization of contemporary England and America. It was far more difficult to conceive of the latters' development than the formers', so that the crux of the issue they faced was: beginning with culture as we know it, can it be derived from the antecedent period and that in turn from the preceding period? The project of tracing cultural evolution coincided with progressive development, and it is important to see that the particular way in which the problem of the evolutionists was phrased was an important factor in their acceptance of the norms of western civilization as the measure of progress. Pointing out the coincidence of the evolutionists' task and the necessity of maintaining the theory of cultural progress in the undertaking of that task does not nullify their ethnocentrism; it does make the standard of progress which they used more intelligible and less arbitrary.

The common ground occupied by Tylor, Lubbock, and Morgan is an adequate indication of the tenets of the evolutionary school of ethnology. Inaugurating a mode of thinking, they were its pioneers and, although not the only cultural evolutionists of the period, they were the principal representatives of the attitudes characteristic when evolution was joined to ethnology. Beginning with the theory of cultural development, the cultural evolutionists were distinguished by their acceptance of the principle of uniformitarianism, the use of the comparative method, a belief in the unity of mankind (psychological and biological), the idea of a basic similarity in the course of social evolution, and an insistence that cultural change has, in general, been a progressive movement.

The qualifying phrase, "in general," needs to be emphasized because, for all their concern with progress, the evolutionists never subscribed to a theory of "guaranteed," "automatic," or "necessary" progress. They were prepared to recognize degeneration, although they thought it more likely to be true of individuals than of cultures and true of culture only under limited circumstances, neither occurrence invalidating the general tendency toward progress. A set of qualifying, environing circumstances was always accessory to cultural advancement, and the essential conviction of the evolutionary school was, not that progress was inevitable and uniform, but that the condition of progress was indissociable from the nature of cultural evolution.

In addition, it should be remembered that running through the works of Tylor, Lubbock, and Morgan is a consciousness that the evolutionary method is the mainstay of naturalism in the social sciences. Spencer shared their conviction of the efficacy of evolution for science, although he ultimately based his historicism on a mechanistic physics, but the evolutionary anthropologists developed no such cosmic scheme as the "Synthetic Philosophy." Tylor justified his belief in universal law by citing the conclusions of the physical sciences, but neither he, Lubbock, nor Morgan made a formula of progress from a physical principle or identified progress with the "survival of the fittest." They were all acquainted with the theories of Darwin and Spencer, and each of them casually nodded in recognition of competitive civilizations and competitive cultural traits, but persistence was not enough to measure progress for the evolutionary ethnologists; they passed on to a close analysis of the process of cultural development.[40]

[39] Morgan, Lewis H., *Ancient society*, 29–30, New York, Henry Holt, 1877.

[40] In concluding *Pre-historic times* Lubbock used a metaphor from biological Darwinism and a quotation from Spencer's *A theory of population* to support his optimistic projection of continuing progress in the future, and Tylor obviously had Darwin or Spencer in mind when he wrote: "History within its proper field, and ethnography over a wider range, combine to show that the institutions which can best hold their own in the world gradually supersede the less fit ones, and this in-

The origin of culture to which they traced man's social life was the ideal limit of their inquiry, and between that beginning point and modern civilization arbitrary causes and imaginary effects would be eliminated by the twin principles of evolution and uniformitarianism. Theirs was a theory by which man acquired culture as a member of society, a theory of culture by accumulation, sloughing, and descent, to which the persistence of what Tylor called "survivals" gave witness. Every culture or segment of culture, Assyrian or Polynesian, whether art, technology, religion, or law, was interpreted as fitting into an evolutionary scheme. That they found this scheme to be unilinear and general is no reason for including among the tenets of the evolutionary platform a theory of independent cultural development, as the diffusionist Elliot Smith tended to do. Rather, the biological and psychic similarity of mankind was associated with general patterns of cultural evolution, as the following excerpts from Morgan and Tylor indicate:

> It may be remarked finally that the experience of mankind has run in nearly uniform channels; that human necessities in similar conditions have been substantially the same; and that the operations of the mental principle have been uniform in virtue of the specific identity of the brain of all the races of mankind.[41]
>
> To general likeness in human nature on the one hand, and to general likeness in the circumstances of life on the other, this similarity and consistency may no doubt be traced, and they may be studied with especial fitness in comparing races near the same grade of civilization. Little respect need be had in such comparisons for date in history or for place on the map.[42]

Lubbock was probably more responsible than either Tylor or Morgan for giving the evolutionists a reputation for ignoring diffusion in social growth, but this is primarily true of the Lubbock of *The Origin of Civilisation*, and it is distinctly not true of Lubbock's other major work, *Pre-Historic Times*. As suggested earlier Lubbock undoubtedly regarded the diffusionist approach with suspicion because the attributing of cultural invention to a few select groups seemed to call into question the ability of any people to produce a civilization. Although the pioneer evolutionists were dragged into the diffusionist controversy, their divergent and varied statements on cultural borrowing put the evolutionary school neither squarely on one side or the other. A wholesale and dogmatic stand on so large a question was extremely vulnerable, and the evolutionists seemed to appreciate this.

The more essential consideration, whether culture developed according to Lubbock's theory of indigenous evolution or according to Tylor's statement that "Civilization is a plant much oftener propagated than developed,"[43] was that civilization evolved. And this view of civilization accommodated both the theory of independent cultural development and the theory of cultural diffusion from a few primary sources, for the derivative character of civilization would hold good for either approach. The diffusionists were not impressed with man's inventive powers and put a correspondingly large burden on man's ability to acquire from others what he had not created. The knack of picking up where others had left off, whether those others were remote or near in space or time, led directly to the continuity in cultural evolution. Just such continuity and cumulative growth was the primary theme of the evolutionists. They could develop it and take into account both cultural loans and cultural invention.

6. CULTURE AND THE UNITY OF MANKIND

Because, to the evolutionists, culture was a unity both in subject matter and in scheme of development, variations in customs, institutions, and beliefs did not contradict their doctrine "that different races in similar stages of development often present more features of resemblance to one another than the same race does to itself, in different stages of its history."[44] Nor did cultural diversity weaken the doctrine of progress when Morgan united progress with evolution: "Progress has been found to be substantially the same in kind in tribes and nations inhabiting different and even disconnected continents, while in the same status, with deviations from uniformity in particular instances produced by special causes."[45] Recogniz-

cessant conflict determines the general resultant course of culture." Morgan, too, in *Systems of consanguinity* referred to "that inexorable law which adjusts numbers to subsistence in given areas . . . ," but to none of their work is the struggle for survival and the survival of the fittest anything more than incidental. It is the continuity of evolution which engrossed them. Lubbock, *Pre-historic times*, 567 and 576. Tylor, *Primitive culture* 1: 69. Morgan, Lewis H., *Systems of consanguinity*, 192, Washington, Smithsonian Institution, 1871.

[41] Morgan, *Ancient society*, 8.

[42] Tylor, *Primitive culture* 1: 6.

[43] *Ibid.*, 53.

[44] Lubbock, *The origin of civilisation*, 7.

[45] Morgan, *Ancient society*, 18.

ing that customs varied among different peoples at different times, the evolutionists brought to the foreground the sequential pattern of cultural evolution.

Tylor's survey of religion and Morgan's analysis of government are striking illustrations of disparate practices harmonized by evolutionary trends. Tylor framed a minimum definition of religion wide enough to include religious beliefs so different from those of modern Christendom that his contemporaries, including Lubbock, claimed that they were not religions at all. Finding the common bond of all religion to be a "belief in Spiritual Beings,"[46] Tylor pieced together the rudimentary animism of primitive tribes and the theology of modern Christianity in a single development, saying, "Animism characterizes tribes very low in the scale of humanity, and thence ascends, deeply modified in its transmission, but from first to last preserving an unbroken continuity, into the midst of high modern culture."[47]

Morgan, using the same evolutionary method, distinguished two fundamental types of political organization, the first based on personal relations (*societas*) and the second based on territory and property (*civitas*). These were his minimum definitions. Under the category of *societas* Morgan placed the gens, phratry, tribe, and league, and in the transition from one to another and in the evolution of *societas* from *civitas* he found a universal plan of political development. Discovering that the sequence of Iroquois political institutions constituted a veritable gamut which all governmental organization had traveled, Morgan re-echoed Tylor's observation that the "character and habit of mankind at once display that similarity and consistency of phenomena which led the Italian proverb-maker to declare that 'all the world is one country.'"[48]

The Grecian gens, phratry, and tribe, the Roman gens, *curia*, and tribe find their analogues in the gens, phratry, and tribe of the American aborigines. In like manner the Irish *sept*, the Scottish *clan*, the *phrara* of the Albanians, and the Sanskrit *ganas*, without extending the comparison further, are the same as the American Indian gens, which has usually been called a clan.[49]

Rather than exhibiting a diversity of practices, all of which were self-contained, the cultural evolutionists absorbed into one general scheme transient phases of what might otherwise have been independent units. A series of ethnic periods from savagery to civilization, "each the outcome of previous history, and about to do its proper part in shaping the history of the future,"[50] appeared vastly more important than a single or isolated culture. For this reason the idea of cultural relativism developed by the second generation of evolutionists is a disrupting intrusion on the twin themes of unity and progress characteristic of the older evolutionary ethnology.

Distinguishing the province of culture from that of physical anthropology, Tylor, Lubbock, and Morgan agreed that the science of culture had its own legitimate subject matter and, with Spencer, included within its general domain mythology, religious beliefs and ceremonies, political institutions, marriage and kinship organizations, technology, art, language, and morality. Understanding culture as a generic term designating a coherent body of material, they grouped customs, artifacts, and beliefs under one comprehensive category of social life. In the unity of mankind they found a justification for the comparative method and for discourse on universal patterns of cultural evolution. Cultural traits remained cultural traits irrespective of the portion of the world or of the period of time from which they were drawn only on the condition that man remained basically the same. Darwin's hypothesis that species of the same genus are descended from a single, primitive stock was consistent with the prospects of universal progress and with the evolutionists' belief that the races of men were not so radically different that the homogeneity of culture was spoiled.

By the time that Darwin published *The Descent of Man* (1871) the controversy over the evolution or creation of species had become an issue equally vital to biologists and cultural anthropologists. The question of whether man was a single species or several had outgrown the pre-Darwinian disagreement between monogenists and polygenists, and after the events of 1858–1859 the dispute centered on the more fundamental problem of whether any species whatever had been created.[51] Darwin, challenging the dogma

[46] Tylor, *Primitive culture* **1**: 424.

[47] *Ibid.*, 426.

[48] *Ibid.*, 6.

[49] Morgan, Lewis H., *Houses and house life of the American aborigines*, 1–2, Washington, Government Printing Office, 1881.

[50] Tylor, *Primitive culture* **1**: 1.

[51] "The whole tendency of modern science is to thrust the origination of things further and further into the background; and the chief philosophical objection to Adam being, not his oneness, but the hypothesis of his special creation; the multiplication of that objection ten-

of either one or multiple creations, predicted "that when the principle of evolution is generally accepted, as it surely will be before long, the dispute between the monogenists and the polygenists will die a silent and unobserved death."[52] Thereafter those who held the evolutionary theory were careful to specify that their belief in the basic similarity of man was "not necessarily the same thing as the descent from a single pair."[53]

Huxley cautiously avoided overstating the evidence for the unity of man, partly on the ground that in the determination of traits the line between strictly physiological characteristics and social environment is thinly drawn. Nevertheless, on the evolutionary hypothesis he asserted that the specific physical differences between men were "so small, that the assumption of more than one primitive stock for all is altogether superfluous."[54] Darwin's conclusion in *The Descent of Man* was identical with Huxley's. Darwin noted the physiological similarity of the races of men and interpreted the difficulty of distinguishing species from varieties and the admittedly arbitrary character of such distinctions as evidence of descent from a common progenitor. But he did more. The alignment between the new biology and the new anthropology was such that Darwin cited *Researches into the Early History of Mankind and Pre-Historic Times* as cultural testimony to biological evolution. Without invoking the authority of Darwin and returning the compliment, Tylor concluded on ethnological grounds that "the wide differences in the civilization and mental state of the various races of mankind are rather differences of development than of origin, rather of degree than of kind."[55] Lubbock went ahead to recognize that "the preceding argument [*Pre-Historic Times*] assumed"[56] the unity of the human race and to authenticate that assumption on biological grounds. But the cultural evolutionists were timorous about entwining the specific details of Darwinism with a belief which they felt was independently warranted by ethnological data. The similarity of conclusions is nonetheless striking.

It is almost tautological to point out that it was *man* and the "experience of mankind," not men and their experiences, which figured most prominently in studies disclosing unilinear cultural growth. The evolutionary ethnologists were aware of the social and biological differences among men, but with their attention on the implications of evolution and progress, they insisted that there was no reason to make those differences paramount. With a universal cultural history to support them, it was easy to suppose that the divergence of races from a primitive stock was less important than the common ancestry which those races shared. Repeatedly affirming their belief in the unity of the human species and the basic similarity of man, they could not accept the polygenetic views of men like Agassiz (whose theory Morgan's friend McIlvaine paraphrased as the theory that men "originated at different centres, in swarms like bees"[57]), for they would have made a hodgepodge of a unilinear cultural development. To have treated races as separate species, as some proposed to do, would have introduced serious qualifications in an otherwise homogeneous subject matter and would have complicated ethnology with a whole group of physiological variables. The adequacy of the comparative method would have been called into question, and the whole structure of social evolution built on comparative techniques would have tottered. Neither racism nor relativism had a place in an ethnology based on the development theory of culture, for the unity of mankind and the unity of cultural evolution were geared too closely together in the panorama of progress for the evolutionists to be diverted by individual social contexts or by contrasts of blood fused in a basic continuity.

Racism was not only generally absent from the evolutionary school; it was tacitly forbidden, and this was a significant fact in a century in which brachycephalic and dolichcephalic indices were used to indicate a great deal more than cranial measurements. Tylor said quite distinctly:

> These pages [*Primitive Culture*] will be so crowded with evidence of such correspondence among mankind, that there is no need to dwell upon its details here, but it may be used at once to override a problem which would complicate the argument, namely, the question of race. For the present purpose it appears both possible and desirable to eliminate considerations of hereditary varieties or races of man, and to treat mankind as homogeneous in nature.

fold is, whatever it may look, an increase, instead of a diminution, of the difficulties of the case." Huxley, T. H., *Man's place in nature*, 247, New York, D. Appleton, 1894.

[52] Darwin, *The descent of man*, 180.

[53] Lubbock, *The origin of civilisation*, 337.

[54] Huxley, *Man's place in nature*, 248.

[55] Tylor, *Researches*, 372.

[56] Lubbock, *Pre-historic times*, 563–567.

[57] Foreman, Edward R. (ed.), *The Rochester historical society* **2**: 54, Rochester, New York, Rochester Historical Society, 1923.

though placed in different grades of civilization. The details of the enquiry will, I think, prove that stages of culture may be compared without taking into account how far tribes who use the same implement, follow the same custom, or believe the same myth, may differ in their bodily configuration and the colour of their skin and hair.[38]

Undoubtedly because of the tenets they accepted and the conclusions which followed from them, the authors of *Ancient Society*, *Primitive Culture*, and *The Origin of Civilisation* were predisposed against identifying social differences with innate bodily differences. On the presumption of the physiological affinity of races, the environments produced by specific cultures and the natural world were the only variables included in the evolutionary platform. It is therefore not surprising that the role and power of culture in determining human behavior was frequently implicit and always taken for granted. In Spencer's work, where sociology merged into anthropology, the idea was made clear and overt, and it came to underlay Sumner's *Folkways* and that part of Veblen's institutional economics which turns on the crucial "point of view."

The principle of the psychic unity of mankind was closely associated with the biological affinity of races, although it had a different source. The fundamental similarity of the human mind served essentially the same purpose in the evolutionary system as its Darwinian counterpart. Both stood behind the comparative method and both served to make intelligible the correspondence of social usages and tools widely dispersed in time and space, but one is unmistakably an offshoot of Darwinism and the other is a debt owed to the German anthropologist Bastian. Psychic unity need be distinguished only because the way in which it was phrased bore directly on the concept of culture, and, in turn, the concept of culture was integral to the idea of cultural progress. Bastian and two of his countrymen, Waitz and Klemm, were frequently quoted in the works of Tylor and Lubbock, and this idea which loomed so large in Bastian's theory of cultural development was particularly evident in the thinking of Morgan. Morgan's proclivity for interpreting social evolution as the growth of mental "germs" or "ideas" might well have stemmed from Bastian's "elementary ideas" (*Elementargedanken*), although neither Morgan nor Bastian made man's innate psychological equipment independently determine the course of cultural history.[39] Nevertheless, all men began with a common mental disposition, and this being granted, the differences among cultures could not be resolved into what was originally an undifferentiated unity. *Völkergedanken*, in Bastian's language, must then be a response to varying stimuli, geographical and social, and so by a quite different route the same conclusion was reached, that human nature or the human race, for ethnological purposes, was by and large a constant. Although Bastian was a vigorous opponent of Darwin's biological transformism and his view of man had another origin, the unity of man, in Bastian's as in the evolutionists' hands, was a way of making cultural growth a natural uniformity while emphasizing the extraorganic factors determining cultural content.

The evolutionary program as a group of interlocking and mutually supporting principles comprises, in modern parlance, a "universe of discourse," a consistent pattern of ideas, each of which conditions and defines the others. On this level of analysis it is almost impossible to distinguish cause and effect, for temporal sequence is irrelevant. And an argument in these terms is likely to move in a circle, so that there is always lurking about an exposition of this sort the suspicion that what is being said has been said before or said in only slightly different words.

The use of the comparative method was essential for the gradation of cultures in the pattern of progressive change, and the principle of the psychic and biological unity of mankind permitted such an analysis to move solely on a social level. It rationalized, of course, an excessively simple theory of the unilinear development of culture and predisposed Lubbock to emphasize a theory of independent social evolution. Not possessing the material and information requisite for tracing cultural development to its origin, the evolutionists found a useful analogy between contemporary and prehistoric "savages." The parallel which they saw between primitive man and his

[38] Tylor, *Primitive culture* 1: 7. See also *Researches into the early history of mankind*, 54.

[39] "The human mind, specifically the same in all individuals in all the tribes and nations of mankind, and limited in the range of its powers, works and must work, in the same uniform channels, and within narrow limits of variation. Its results in disconnected regions of space, and in widely separated ages of time, articulate in a logically connected chain of common experiences. In the grand aggregate may still be recognized the few primary germs of thought, working upon primary human necessities, which, through the natural process of development, have produced such vast results." Morgan, *Ancient society*, 255.

modern analogue depended upon Lyell's principle that past events be explained as the consequences of causes still acting in the present. The assumption of such an analogy was of paramount importance in reconstructing cultural history. In the absence of specific data, from the evolutionists' point of view, some variation or application of the principle of uniformitarianism offered the only way out, at least if the evolutionary project was to be carried through. It fitted the remainder of the evolutionary program ideally. A unilinear cultural theory made the analogy likely, and the analogy of course encouraged just such a theory of cultural development.

Morgan, accounting for the neat and simple order which he found in the social development of man, wrote:

> Since mankind were one in origin, their career has been essentially one, running in different but uniform channels upon all continents, and very similarly in all the tribes and nations of mankind down to the same status of advancement. It follows that the history and experience of the American Indian tribes represent, more or less nearly, the history and experience of our own remote ancestors when in corresponding conditions. Forming a part of the human record, their institutions, arts, inventions and practical experience possess a high and special value reaching far beyond the Indian race itself.[60]

Tylor and Lubbock adopted the same point of view. One notes that the full title of Lubbock's first major ethnological work was *Pre-Historic Times as Illustrated by Ancient Remains and the Manners and Customs of Modern Savages*. Almost as an echo Tylor said:

> In the following chapter, on the Development of Culture, an attempt is made to sketch a theoretical course of civilization among mankind, such as appears on the whole most accordant with the evidence. By comparing the various stages of civilization among races known to history, with the aid of archaeological inference from the remains of pre-historic tribes, it seems possible to judge in a rough way of an early general condition of man, which from our point of view is to be regarded as a primitive condition, whatever yet earlier state may in reality have lain behind it. This hypothetical primitive condition corresponds in a considerable degree to that of modern savage tribes, who, in spite of their difference and distance, have in common certain elements of civilization, which seem remains of an early state of the human race at large.[61]

[60] *Ibid.*, vii.

[61] Tylor, *Primitive culture* 1: 21. ". . . The higher culture being clearly traced back to what may be called the middle culture, the question which remains is, whether this middle culture may be traced back to the lower culture, that is, to savagery." *Ibid.*, 32.

The evolutionary method worked both ways, and the evolutionists, reversing the dictum that "the past is continually needed to explain the present,"[62] were just as confident that the cultures of modern primitives gave broad hints of the ways of primeval man. The unity of cultural development made working from the known to the unknown feasible, and the principle of uniformitarianism practically guaranteed it.

Since the crucial decades of 1860 and 1870, when the evolutionary program was initiated and developed, there has been a basic shift in the predominant perspective in cultural anthropology. An evolutionary ethnology which had as its maxim that "all the world is one country," with the tacit recognition that its vineyard was the culture of mankind, has largely given way to functionalist monographs and detailed ethnographical studies. The catholic scope of the ethnology which marked the beginning of "the science of culture" has been seriously circumscribed; the universe of culture has been dismembered into provinces and principalities more appropriate for specialized study and for the collection of empirical data. Since the advent of systematic field-research, the grand sweep fostered by the evolutionary point of view has fallen into disrepute as speculative philosophy. With the new currents that have enveloped this later anthropology there has developed a frame of mind that has gradually undermined the evolutionists' belief in social progress, a belief that is now labeled as peculiarly typical of the nineteenth century. This more modern frame of mind is best expressed as one centering around the concept of cultural relativity; its concurrent appearance with "functionalism" in anthropology and sociology speaks of more than a tenuous connection, for the notion of cultural context undergirds cultural relativity. The sources of human conduct, the legitimacy and authentication of values or norms, including the norm of progress, are read with increasing insistency in terms of social conditioning; human behavior is made a function of social context.

7. E. B. TYLOR: CULTURE AND THE CONCEPT OF "SURVIVALS"

Of the men who inaugurated the search for cultural origins and the evolutionary quest for progress, Tylor and Morgan seem more likely to survive than Lubbock, who is now remembered largely as the man who added "paleolithic" and

[62] Tylor, *Researches*, 2.

"neolithic" to the archaeological vocabulary. The case with Morgan and Tylor is somewhat different. For their work not only illustrates the body of tenets shared by the evolutionists, but it contains certain features bearing directly on cultural progress and the growth of contextualism, which proved so inimical to their doctrine of progress and to the evolutionary point of view. In a consideration of the details of the systems of Tylor and Morgan, the former's concept of survivals and the latter's economic contextualism seem the most fruitful central ideas about which to organize a survey of their work.

"In the spring of 1856," Tylor wrote in *Anahuac*, "I met with Mr. Christy accidentally in an omnibus in Havana."[63] It was an auspicious meeting. The distinguished archaeologist Henry Christy was en route to Mexico intending to add to his pre-Columbian collections, and Tylor, then twenty-three, had left the family brass foundry to travel for his health. He had no immediate plans, and Christy asked Tylor to go on with him to Mexico.[64] Tylor's apprenticeship in anthropology began, and the first book that he wrote, *Anahuac* (1861) was compiled from the letters and memoranda written on their journey through central Mexico. *Anahuac* is an amiable and leisurely travel book, with an occasional ethnological turn, an amateur's speculations about Mayan art, the sources of Aztec civilization, and the "wonderful similarity of character among the stone weapons found in different parts of the world."[65] After four months with Christy seeing the temple-pyramids of Teotihuacan, obsidian mines, and the ruins of Xochicalo, Tylor began a career which ended at Oxford with his holding the first professorship of anthropology. The step from *Anahuac* to *The Early History of Mankind* (1865) was the first in that direction. With the publication of *Primitive Culture* (1871), Tylor's rank as the foremost British cultural anthropologist was established, and he rapidly gained the reputation which led Max Müller to call anthropology "Mr. Tylor's science." He became a Fellow of the Royal Society and was later the first president of the Anthropological Section of the British Association.

The anthropologist R. R. Marrett, one of Tylor's students at Oxford, called Tylor an "orthodox Darwinian," and the first time that Tylor made a statement on physical anthropology, in his popular, and general, *Anthropology* (1881), he followed the line of orthodoxy in a chapter on "man's place in the animal world" called "Man and Other Animals." Huxley helped in its preparation. Certainly the evolutionary idea was decisive in his ethnology, but the sources on which Tylor drew for the development theory of culture are reminders that there were other, independent, phrasings of evolution than the Darwinian.[66] These sources are not difficult to discern; at least some may be credited to the English historian Buckle and the continental anthropologist Gustav Klemm, whose concept of culture anticipated Tylor's.[67]

On the first page of *The Early History of Mankind* Tylor defined civilization and broached the concept with which his name has been associated ever since, "survivals," or what he first called "superstitions." The definition of civilization led to a consideration of cultural traits that were particularly marked off as "standing over" from an ancient heritage. Since culture was a legacy, all civilization or culture was in a broad sense a survival from the past, and a cultural deposit accumulated over an immensely long period would bear signs of its archaic origins. The evolution and progress of culture was a history of adding the new as well as losing the old "knowledge, belief, art, morals, law, and custom," that comprised the "complex whole" of culture, and "superstitions" were the remnants of earlier civilizations relegated to the ceremonial fringes around the hard core of the substantial culture.

His later work *Primitive Culture* also began with a definition of civilization, or of what he then preferred to call *culture*, and two chapters were devoted to the exposition of survivals and

[63] Tylor, Edward B., *Anahuac: or Mexico and the Mexicans, ancient and modern*, 1, London, Longmans, Green, 1861.

[64] Two years later Christy began the explorations, with Edouard Lartet, recorded in *Reliquiae aquitanicae* (1865–1870), of the Périgord caves, multiplying the evidence of the existence of Paleolithic man. See Tylor's *Researches*, 199–200.

[65] *Ibid.*, 102.

[66] In the preface to *Primitive culture* Tylor wrote: "It may have struck some readers as an omission, that in a work on civilization insisting so strenuously on a theory of development or evolution, mention should scarcely have been made of Mr. Darwin and Mr. Herbert Spencer, whose influence on the whole course of modern thought on such subjects should not be left without formal recognition. This absence of particular reference is accounted for by the present work, arranged on its own lines, coming scarcely into contact of detail with the previous works of these eminent philosophers."

[67] See Robert H. Lowie, *The history of ethnological theory*, 12, New York, Rinehart, 1937.

their place in evolutionary ethnology. And instead of as a "development," Tylor had begun to write of cultural changes as an "evolution." The definition of culture at which Tylor arrived, one that was modified and extended from *The Early History of Mankind*, contained a significant addition. Culture consisted of a "complex whole . . . acquired by man as a member of society."[68] At the same time that Tylor specified that culture was a community product, he assumed that it could be studied as a collective, societal phenomenon because, for all its complexity, it was a whole with characteristics general and predominant enough to make it the manageable subject of the science of culture. What accounted for that homogeneity Tylor was not sure, but that entire populations were somehow induced "to unite in the use of the same language, to follow the same religion and customary law, to settle down to the same general level of art and knowledge"[69] seemed evident. He thought of social norms figuratively as a "tacit consensus or agreement," and without further probing the consistencies that typify cultures, he simply recognized that man as a member of society tended to follow the pattern of the society in which he lived.

By specifying that culture or civilization was acquired by man as a member of society, Tylor did not mean that culture was developed independently of a real and existing group of individuals or that some specific group of individuals was not always the legatee of civilization. He did mean that culture was more appropriately considered from its communal rather than its individual aspect and that generalizations about it could be made without constant reference to variations from social norms. Men drew on the resources of a social heritage because they were members of a group; and because a cultural legacy was vested in the society, any addition to that legacy inevitably began from the cultural continuum and ended there, or counted for nothing in social history. Tylor recognized that the processes of social diffusion could convert individual inventions and novel beliefs into a communal possession and that social change always had a human agency. But it was the continuity of the social heritage that made any innovation finally "independent of the fate of its originators."

> What is produced in some limited district is diffused over a wider and wider area, where the process of effectual "stamping out" becomes more and more difficult. Thus it is even possible for the habits and inventions of races long extinct to remain as the common property of surviving nations; and the destructive actions which make such havoc with the civilizations of particular districts fail to destroy the civilization of the world.[70]

The community which men share, Tylor naturally insisted, is not limited by contemporary society, for every society is continuous with a preceding one. This double sense of community, community as an evolving society and community as a group in which private and personal experience may become widespread and social, made culture intelligible and progress possible. From the broad perspective of the development theory, any single contribution seemed trifling in comparison with the vast accumulation of societies during previous innumerable ages. "It needs but a glance into the trivial details of our own daily life to set us thinking how far we are really its originators, and how far but the transmitters and modifiers of the results of long past ages."[71] Spencer might have said this somewhere in his sociology, but none of the evolutionists would have found that the laws of the physical universe, codified in an evolutionary formula, prescribed the form of cultural development and reduced human agency to the movement of a marionette. Without confusing moral right with physical order, Tylor nevertheless suggested in his own systematic ethnology that cultures gravitate around dominant centers and that the complex whole of culture is brought into a general accord, with, of course, the loss of traits which do not fit.

As evidence of the evolution of culture and as an ethnographical key to the recovery of primitive tradition, survivals assumed a salient place in Tylor's ethnology. His theory of cultural development emphasized that civilization was a social inheritance; and the imprint of its primitive history was tangled in persistent myths, folklore, games, proverbs, superstitions, and ceremonies far removed from their original source. These relics of an earlier time lingered on in modern times as dim reminders of an antiquity and continuity of culture. They also indicated something important about progress: that progress was not only positive innovation, "but negative in taking away something belonging to an earlier state of things."[72]

The terms "survivals" gained a general cur-

[68] Tylor, *Primitive culture* 1: 1.
[69] *Ibid.*, 10.
[70] *Ibid.*, 39.
[71] *Ibid.*, 17.
[72] Tylor, *Researches*, 2.

rency among the cultural evolutionists. Lubbock adopted it to cover "certain customs . . . which we find lingering on in civilized communities . . . , a page of history," telling "a tale of former barbarism. . . ."[73] But before Tylor's *Primitive Culture* appeared, Morgan was independently exploring a new field of anthropology in *Systems of Consanguinity and Affinity of the Human Family*, and the evolutionary hypothesis was leading him just as certainly as it led Tylor to interpret traditional usages as archaic remnants of an earlier social condition. Morgan found that kinship terminologies grew out of systems of marriage and that long after the disappearance of the familial relations underlying them, obsolescent ways of naming kin held on, leaving a record of social conditions which no longer existed. This technique of tracing the continuity of social history might have come from a page of *The Descent of Man*. While Darwin was saying that vestigial organs point to the anatomical evolution of man, Tylor, Lubbock, and Morgan were using rudimentary cultural traits as evidence of man's developing civilization. The same logic inheres in both, and Darwin's statement that "we can hardly suppose that they [rudimentary organs] were developed under the conditions which now exist"[74] is paralleled by Tylor's reasoning that survivals exist in "a new culture which certainly would never have brought them in. . . ."[75] Survivals were "landmarks in the course of culture" that hinted at previous and different customs and beliefs, clues to be followed for the revelation of primitive stages of cultural evolution.

In spite of the fact that in his theory of civilization all culture is virtually a heritage, a survival, Tylor distinguished only parts of a tradition as "survivals." What determined this selection? That a survival is inherited is not the key, since the body of culture itself is a legacy. It is true, nonetheless, that Tylor listed tenacious persistence, "the lasting on of fixed habits of mankind," as one of the qualities of a survival. Persistence alone was an insufficient differentia, but coupled with the preconceptions of the evolutionary school, the tenacity of the received tradition became more meaningful than it otherwise would and explained why Tylor considered survivals, at worst, a positive menace. ". . . Civilization has to contend . . . with survival from lower levels. . . ."[76] This was a serious matter only because of Tylor's working generalization that remoteness in time is correlated with primitiveness of culture, a generalization having as its corollary that progress is assumed with evolution. Because evolution takes time and no tribe or nation occupies the level of culture from which it began, advanced civilizations are also later civilizations, and all cultures carry the burden of a tradition more or less remote in time and antiquated in orientation. Advancing civilization inevitably arrives at the stage where survivals are a part of its social legacy, and they are likely to be increasingly out of harmony with the prevailing scheme the longer they persist. The effect of thus bringing together "evolution," "progress," and "survivals" was to introduce the theme of contextualism, and what may be particularly important is that there is no way of framing Tylor's definition of survivals without finally resorting to contextualism, an interpretive technique based on appropriateness to a certain set of generating and sustaining conditions.[77]

The impression that survivals are merely old, skirts the full meaning of Tylor's use of the concept. Longevity is a genuine mark of survivals because it measures more than a simple lapse of time in the life-history of a cultural trait. For Tylor, survivals had lost their original meaning and vitality when they appeared in an uncongenial cultural scheme, and their antiquity was one indication of a shift from the core to the periphery of a civilization. From Tylor's optimistic point of view, ". . . the survival of opinions in the midst of conditions of society becoming gradually estranged from them, and tending at last to suppress them altogether . . ."[78] had but one conclusion. Survivals were doomed. He, therefore, ordinarily identified them as dwindling and tenuous, sinking into a decayed remnant of another complex whole of culture, holding on in ceremonies, children's games, and proverbs. ". . . The special wonder about it is that the change and revolution of human affairs should have left so

[73] Lubbock, *The origin of civilisation*, 341.
[74] Darwin, *The descent of man*, 11–12.
[75] Tylor, *Primitive culture*, 1: 71.
[76] *Ibid.*, 69.
[77] When Tylor discussed progress and degradation in his *Anthropology*, he explained the failure to adopt obvious improvements by noting that ". . . the highest arts and the most elaborate arrangements of society do not always prevail, in fact they may be too perfect to hold their ground, for people must have what fits with their circumstances." Tylor, Edward B., *Anthropology, an introduction to the study of man and civilization*, 18–19, New York, D. Appleton, 1891.
[78] Tylor, *Primitive culture* 1: 112.

many of its feeblest rivulets to have run so long." [79]

In the changed circumstances with which survivals are out of harmony, "a new culture . . . presses hard to thrust them out," [80] but survivals nevertheless manage to hold on. It then follows that, by virtue of the evolution and continuity of civilization, communities fall heir to a tradition which is not a consistent piece; inconsistent because the indispensable prerequisite to any culture is a pre-existing culture and because conditions change, almost automatically insuring that in a cultural deposit there is awkward jarring of discordant segments. If what is transmitted is a "survival," then the circumstances in which it originally appeared no longer exist in full vigor to maintain the basic features of a cultural pattern intact, and the holdover, without the support of a sympathetic social environment, is the relic Tylor called "survival."

Tylor made here the standard of validity a contextualistic norm; in one culture certain habits and usages were integral features of the predominant order, while in a new set of conditions those same habits and usages were archaic remnants standing in the relation of what modern sociologists would call "dysfunctions" to the current drift of social organization. Tylor's theory of survivals contained more than a suggestion of cultural relativism and contextualism. He had called attention to the changing complexion of social situations with which norms of adequacy change, too, and, disavowing a biological explanation of cultural differences, he had pointed out that there was such "regularity in the composition of societies of men, that we can drop individual differences out of sight, and thus can generalize on the arts and opinions of whole nations. . . ." [81] Cultural grounds sustained the variety of human behavior. In Tylor's *Anthropology* (1881), his frequently overlooked contribution to the International Scientific Series, custom was made the lord of conduct and the pressure of public opinion the ally of custom, while custom gave "the rule as to what is to be done or not done in most affairs of life." [82] Without ever recanting his faith in progress, Tylor carries this theme to its relativistic conclusion:

> Thus communities, however ancient and rude, always have their rules of right and wrong. But as to what acts have been held right and wrong, the student of history must avoid that error which the proverb calls measuring other people's corn by one's own bushel. Not judging the customs of nations at other stages of culture by his own modern standard, he has to bring his knowledge to the help of his imagination, so as to see institutions where they belong and as they work. Only thus can it be made clear that the rules of good and bad, right and wrong, are not fixed alike for all men at all times.[83]

But, like the cultural relativism in Spencer's sociology, the contextualism of Tylor's anthropology is subordinate to an evolutionary sequence spelling not only development but progress.

The power of a cultural trait to persist in its original form could never have been the test of progress for Tylor. In the continuity of culture one scheme developed into another, effectively eliminating sheer persistence as the criterion of social advancement. It is correct to say that by and large Tylor attributed a beneficent trend to cultural development; it is equally correct, however, to say that he and the evolutionists recognized that a progressive movement was as likely to stop short in Iroquois barbarism or in a lower grade of savagery as not, but the recognition did not disturb their confidence in progress. In their view, primitive culture was not abnormal or a universal pattern gone astray to be explained as an exceptional case, for they assumed that even a state of savagery was less primitive than what had gone before. Potentiality for further development more accurately describes their measure of progress than persistence or permanence. Although, in tracing the meliorative course of cultural growth, all of the evolutionists approached the stages of social evolution as periods of transition from something lower to something higher, they did not underscore the capacity to evolve as the intrinsic mark of progress, perhaps because it finally depended on a future turn of events. But looking back over the whole cultural sequence, Tylor, Lubbock, and Morgan repeatedly remarked the progressive course of cultural change.

Taking its cue from the theme of gradual change and paying scant attention to the Malthusian struggle for existence, Tylor's idea of progress has only a superficial resemblance to the competitive framework of Spencer's "survival of the fittest." What the future might hold was vaguely sketched, if at all, for none of the evolutionists felt it incumbent upon himself to include in his analysis a blueprint of the good society which he felt lay remotely ahead. The absence of

[79] *Ibid.*, 70.
[80] *Ibid.*, 71.
[81] *Ibid.*, 11.
[82] Tylor, *Anthropology*, 409.
[83] *Ibid.*, 410.

such a prediction may simply be a sign of their tending to the ethnological business at hand, but it is significant that the end of social evolution was not the equilibrium of perfectly balanced, harmonious parts that William James called "the lady-like tea-table elysium represented in Mr. Spencer's *Data of Ethics*."[84] It is furthermore significant that the drama of evolution was not closed with an ending at all, and, lacking a predetermined finis, the evolutionary view of progress is a serious departure from the romantic utopianism of an earlier period. Without the *a priori* finality of dialectical evolution, either Hegelian or Marxian, and without the mechanistic teleology of the Spencerian system, Tylor's evolutionary theory seems closer to the temper of the more recent Darwinism, a temper which another evolutionist, Thorstein Veblen, liked to typify as the scientific attitude of modern times.

Tylor's belief in progress and his sensitivity to the uncertain status of the fledgling science of anthropology discouraged his treatment of survivals beyond such commonplace examples as the ordeal of the Key and Bible, the Midsummer bonfire, and the Breton peasants' All Souls' supper. They would, after all, in time pass away, and why confuse the issue of a science of culture with a partisan anthropology? The gingerly way in which Tylor handled the implications of the survival concept suggests Darwin's timidity in implicating man in *The Origin of Species*—". . . I thought that I should thus only add prejudice to my views." But in Tylor's mind there was no doubt that games, riddles, witchcraft, proverbs, and astrology did not exhaust the list of survivals.[85]

Tylor first used "superstition" to denote "the 'standing over' of old habits into the midst of a new and changed state of things,"[86] and, although the literal etymology was completely accurate, "superstition" was an invidious term, unnecessarily harsh when applied to the games, sports, and rhymes of children. "For the ethnographer's purpose [purposes of science], at any rate, it is desirable to introduce such a term as 'survival,' simply to denote the historical fact which the word 'superstition' is now spoiled for expressing."[87] "Survival" connoted none of the reproach of "superstition" and therefore seemed the preferable term in a scientific analysis, but Tylor closed his chapters on survivals by urging that an interest in "such dead old history" was no mere antiquarian curiosity.

> . . . We have in such enquiries continual reason to be thankful for fools. It is quite wonderful, even if we hardly go below the surface of the subject, to see how large a share stupidity and unpractical conservatism and dogged superstition have had in preserving for us traces of the history of our race, which practical utilitarianism would have remorselessly swept away.[88]
>
> But the opinions drawn from old or worn-out culture are not to be left lying where they were shaped. It is no more reasonable to suppose the laws of mind differently constituted in Australia and in England, in the time of the cave-dwellers and in the time of the builders of sheet-iron houses, than to suppose that the laws of chemical combination were of one sort in the time of the coal-measures, and are of another now. The thing that has been will be; and we are to study savages and old nations to learn the laws that under new circumstances are working for good or ill in our own development.[89]

In the American tradition, a more direct application of the survival concept to the contemporary scene awaited Veblen's analysis of business enterprise. Veblen turned the notion of institutional lag to substantial account in dissecting the economy of a civilization to which the industrial revolution is only the latest comer. He found evidences of an older barbarism and of a temperament more savage than modern in the folklore of pecuniary management. Taking the culture concept and the psychology of habit to his analysis of the leisure class, Veblen had a field day with the manners and rituals of the gaudy rich and those who aped them. Ancient usages had a way of holding on and serving as symbols of prestige, and Veblen delighted to point out that the archaic was not self-evidently meritorious. While Tylor had designated survivals as cultural backwash, tending to be remote and incidental, Veblen uncovered them at the center of contemporary life. But when Veblen labeled the canons which govern adequacy in the sphere of ceremony, he did so largely in the terms hinted at when Tylor noticed that survivals persisted in custom and rite where authority is counted by antiquity, "rather by being old than by being good."

[84] James, William, *Essays in faith and morals*, 168, New York, Longmans, Green, 1943.

[85] "The ethnographer's course, again, should be like that of the anatomist who carries on his studies if possible rather on dead than on living subjects; vivisection is nervous work, and the humane investigator hates inflicting needless pain." Tylor, *Primitive culture* **1**: 158.

[86] Tylor, *Researches*, 221.

[87] Tylor, *Primitive culture* **1**: 72.

[88] *Ibid.*, 156.

[89] *Ibid.*, 158–159.

Tylor, Morgan, and Spencer looked to the evolutionary process for a standard of progress. Conscious of the telltale anthropocentric bias in earlier conceptions of progress, Spencer identified the laws of mechanics with the laws of evolution and equated progress with evolution. Tylor and Morgan, frequently oblivious of their ethnocentrism, appealed to the enlightened standards of the western world as the pinnacle of social evolution. Tylor grouped together "the industrial arts, especially metalworking, manufacture of implements and vessels, agriculture, architecture, etc., the extent of scientific knowledge, the definiteness of moral principles, the condition of religious belief and ceremony, the degree of social and political organization," [90] as criteria of progress. Although he felt more certain of maintaining the advance of civilization in the realm of material and intellectual culture,[91] it is generally conceded that in measuring progress Tylor specifically assumed the standards of Victorian England and that what he meant, for instance, by "the conditions of religious belief and ceremony" was the condition of religious belief and ceremony in the cultivated circles of western Christianity in his time.

8. L. H. MORGAN: ECONOMIC CONTEXTUALISM

In the same wholesale fashion Morgan interpreted the evolution of the industrial arts, government, property, and the family, each in an ascending line of cultural progress. Civilization moved forward as each department of culture advanced beyond the previous stage, and for both Tylor and Morgan cumulative experience was the main vehicle of ubiquitous progress. Only Morgan began to discriminate further the basis of cultural progress in cultural terms. Spencer, of course, had found a rationale for the beneficent order of nature when he finally rested his general law of progress on physics, but Morgan's analysis kept to the social level; and, however haltingly he hinged the complex whole of culture to an economic foundation, Morgan went a long way toward articulating a more specific doctrine of universal improvement. The evolution of the arts of subsistence with which Morgan began his *Ancient Society* forms a bridge to Veblen's *The Instinct of Workmanship and the State of the Industrial Arts*. Veblen distinguished a fundamental dichotomy running through cultural history that turns on science and the machine process and elaborated his "institutional economics" around that distinction. It is not too much to say that his economic theory was a by-product of his evolutionary analysis of social development, and Veblen himself wrote that "in the growth of culture . . . the facts of technological use and want are fundamental and definitive, in the sense that they underlie and condition the scope and method of civilization in other than the technological respect." [92] To understand better the extent to which Morgan interpreted social evolution on an economic basis it is necessary to inspect his cultural studies and their materialistic foundation.

Lewis Henry Morgan's first major work, *The League of the Iroquois* (1851), contains hints of the theme which pervades the last book that he wrote, the book for which he remains most popularly known, *Ancient Society* (1877).[93] It would be false, however, to call Morgan's study of the Iroquois evolutionary or to claim that it proceeds on that basis which is broadly described as the materialistic (economic) conception of history. *The League of the Iroquois* is not concerned with the growth of customs and institutions, much less with their economic foundation. Limited in subject matter to the tribes which made up the Iroquois League, it lacks the universal scope and the universal scheme of cultural evolution characteristic of *Ancient Society*. Precisely it is ethnography, an intensive, sympathetic monograph on a people with whom Morgan had been acquainted since his boyhood in northern New York. But before Morgan had finished writing on the Iroquois, he had incidentally used the comparative method and had begun to see a general form in the evolution of political institutions, a method and a theme which grew increasingly important in his later work. He had noticed that there was a similarity between Iroquois and Greek, Roman, and Hebrew political institutions and that one form of government evolves from an earlier form in a specific sequence.[94] When Morgan claimed that Iroquois

[90] *Ibid.*, 26–27.
[91] *Ibid.*, 27–28.

[92] Veblen, Thorstein, *The instinct of workmanship*, vii, New York, Viking, 1946.

[93] For a short and comprehensive account of Morgan's work, see Leslie A. White's *Lewis Henry Morgan: pioneer in the theory of social evolution* in *An introduction to the history of sociology* (ed. Harry Elmer Barnes), Chicago, University of Chicago Press, 1948.

[94] In *The league of the Iroquois* Morgan read this sequence as the evolution from monarchical to democratic

institutions "contain the sum and substance of those of the whole Indian family,"[95] he erected features of a local organization into types having more than provincial significance. He broached the idea of inclusive cultural schemes by a grand inductive leap on limited evidence. Of course Morgan said more than he knew, but he was later to pin down his claim in the closely empirical study of Indian kinship systems.

Although Morgan came closer than either Lubbock or Tylor to making racial differences a mark of inherent superiority and inferiority, already in *The League of the Iroquois* his chief emphasis was sociological and broad enough to include the physical habitat: "The development of national intellect depends chiefly upon external, reciprocal influences, and is usually proportionate to the vitality and motive which the institutions of a people possess and furnish."[96] Although tainted with the myth of Aryan supremacy, Morgan joined the evolutionary school of ethnologists in holding the common origin and psychic unity of the human species. But all this, including Morgan's occasional references to the relation of property to social evolution and his association of Iroquois social organization with a "hunter state,"[97] is secondary to his main interest, and only the later material of *Systems of Consanguinity* and *Ancient Society* calls for these ideas being noticed at all.

Morgan's next work, *Systems of Consanguinity and Affinity of the Human Family* (1871), requires closer and more serious attention, for it is here that the most characteristic features of his anthropology unmistakably appear. On a business trip to Marquette, Michigan, the prosperous lawyer and railroad investor made the discovery that led him to begin an intensive study of kinship distinctions. Morgan was already familiar with the Iroquois way of naming kin, and when he found that the Ojibwas on the southern shore of Lake Superior used the same elaborate and complex system of distinguishing kin, he was startled. The Iroquois and Ojibwa languages were strikingly different, making the similarity of kinship classification the more remarkable, too remarkable to be put down to coincidence or independent invention. Morgan's hypothesis immediately took shape: perhaps all the Indian tribes of North America used the system that he had found among the Iroquois and the Ojibwas.

He scoured the continent east of the Rockies in person, complaining that his correspondents didn't get the data right and that he had better gather it himself. Looking farther abroad, he enlisted the help of the State Department and the Smithsonian Institution in sending out questionnaires all over the world. His was to be a prodigious survey. General Lewis Cass and William H. Seward, Secretaries of State, were willing to have Morgan's inquiries go out under the official seal of their department to consular and diplomatic representatives. This was no political matter, but Morgan's political affiliation did no harm. In the very period of Morgan's work on kinship systems, he served terms in the New York Assembly and Senate under the Republican banner. American anthropology has seldom been so radical and respectable at the same time. When *Systems of Consanguinity and Affinity* finally appeared, it came into print as a government document under the auspices of a board of regents which included the Vice-president of the United States and the Chief Justice of the Supreme Court.

As one would expect, Morgan never heard from many of his questionnaires, and some had to be resubmitted when the kinship schedules were carelessly filled in. Latin America proved a difficult area from which to get information, and Morgan felt that Roman Catholic bishops showed little enthusiasm for ethnology. From Morgan's point of view, Protestant mission boards showed a more proper spirit. They were energetic in getting Morgan's inquiries to "the field," and the missionaries themselves were conscientious and painstaking, if not always prompt, in supplying the requested information.

As Morgan assembled the schedules of kinship classifications which came in from his question-

government, taking a position substantially similar to Spencer's in *The principles of sociology*. By the time Morgan wrote *Ancient society* he regarded his earlier generalization of the trend of political institutions as a serious error and reversed the sequence.

[95] Morgan, Lewis Henry, *The league of the Iroquois*, 52, New York, Dodd, Mead, 1904.

[96] *Ibid.*, 121.

[97] "Without the influence of cities, which no people construct who live in the hunter state, and the important consequences which result from the aggregation of society into large communities, the government of the Iroquois would doubtless have retained its oligarchical form through many generations. It would have lasted until the people had abandoned the hunter state; until they had given up the chase for agriculture, the arts of war for those of industry, the hunting ground and the fishing encampment for the village and the city." *Ibid.*, 132. Morgan's economic contextualism in the analysis of culture had begun.

naires, he distinguished two fundamental types of relationship systems, which he called "descriptive" and "classificatory." Using these kinship relations as clues, Morgan then explained their diversity by reconstructing the family systems lying behind them and by arranging these family systems in an evolutionary sequence. The political evolution of society in a "regular progression" or in a "natural order," touched on in *The League of the Iroquois*, was extended to kinship, marriage, and the family in a comprehensive and exhaustive analysis in *Systems of Consanguinity*. It placed Morgan squarely among the social evolutionists; and when he raised his earlier, tentative suggestion of successive stages to a universal principle of cultural development (in familial evolution), Morgan joined Lubbock and Tylor in championing unilinear evolution. At the same time Morgan affirmed the unity of human origin, treating races as "a family of nations."

> Mankind, if one in origin [which Morgan never seriously questioned], must have become subdivided at a very early period into individual nations. Unequal progress has been made by their descendants from that day to the present; some of them still remaining in a condition not far removed from the primitive, and now revealing many of the intervening stages of progress. It must be supposed, therefore, that these customs and institutions, taken as a complete series or sequence, must have been of slow growth, and of still slower diffusion amongst the nations, as they progressed in experience; and that they are but the great remaining landmarks of this experience, whilst the mass of minor influences which contributed to their adoption have fallen out of knowledge.[98]

Kinship relationships became ethnological clues for the first time. Morgan's study revealed nomenclatures strangely out of harmony with existing marriage customs, and he tried to reconstruct the kind of family-institution which would make those anachronisms intelligible. In short, Morgan used kinship systems as Tylor had used "survivals" and on the same evolutionary grounds.

When *Systems of Consanguinity* first appeared, Morgan was on a holiday in Europe, relaxing after the rigorous work of getting the book to press. While in England he saw Max Müller and Huxley and had several visits with McLennan and Lubbock, and talked with Darwin. As might have been expected, he also wanted to meet Spencer and "possibly Mr. Maine" (Sir Henry Maine). He knew Tylor's reputation well enough to want to see Tylor, but he couldn't get the name straight. Morgan asked McLennan to introduce him to "Mr. Taylor." When Morgan went to Down, Darwin told Morgan that the relationships in *Systems of Consanguinity* were difficult to follow and that he had advised Lubbock against lecturing on Morgan's new book before a non-technical audience. Although it established his claim to rank among the pioneers of a scientific anthropology, Morgan was completely accurate when he predicted that "It will be a long time before it is much known."[99] Except among professional anthropologists, it has never been a well-known book.

The sheer size of *Systems of Consanguinity* is forbidding and its massive compilation of kinship terminologies discouraging. Even Morgan showed signs of fatiguing under the weight of all those kinship tables. At the end of one labyrinthian exposition, Morgan apologized, almost to complain to himself, for its "tedious minuteness." The patience of the Indians he found particularly touching, and he paid tribute to their "natural kindness of heart" in submitting to so many detailed questions about their relatives. Of course, the book's significance lies neither in its bulk nor its endless tables of kinship systems but in Morgan's interpretation of those nomenclatures as lexical reflections of social organization. In the absence of social conditions which might be supposed to account for the terminology of kinship relations, Morgan's problem was to infer the marriage systems with which the terms were originally associated. Kinship systems lagged. They were "great remaining landmarks" of previous systems of consanguinity and affinity. They were survivals, holding on with enough permanence to suggest obsolete marriage customs, and Morgan was so impressed with their persistence that he supposed that they were passed down from generation to generation in the blood stream. In Morgan's words the result of *Systems of Consanguinity* was:

> A comparison of these systems [of consanguinity], and a careful study of the slight but clearly marked changes through which they have passed, have led, most unexpectedly, to the recovery, conjecturally at least, of the great series or sequence of customs and institutions which mark the pathway of man's progress through the ages of barbarism; and by means of

[98] Morgan, *Systems of consanguinity*, 479.

[99] White, Leslie A. (ed.), *Lewis H. Morgan's European journal, Rochester historical society publication* **16** (2), 370, Rochester, New York, Du Bois Press, 1937.

which he raised himself from a state of promiscuous intercourse to final civilization.[100]

It [*Systems of Consanguinity*] embodies the first collection of facts which tend to show the successive stages of growth of the Family, indicating four of these stages with a good deal of certainty. This I regard as the most remarkable result by far of the investigation.[101]

When Morgan wrote *Ancient Society*, this "remarkable" finding became a theory of social evolution and progress which nominally stood, at least in the first pages of the book, on an equal footing with his theory of economic progress. In *Ancient Society* he wrote that social relations afford a second "line of investigation," analytically distinguishable from the evolution of arts of subsistence, the first line, and indicated that by either of these the progress of human history may be measured. The bifurcation of social evolution into two lines of development, social and economic, which complicates the procedure of *Ancient Society* began in Morgan's study of kinship systems.

Morgan's economic bias in anthropology was not an *a priori* dogma but a persuasion that he came to gradually. Without ever becoming prominent or intruding on the main line of argument in *Systems of Consanguinity*, the correlation between successive cultural levels and their appropriate economies is definitely broached. It was this theme, as it was later and emphatically developed in *Ancient Society*, that particularly attracted the attention of socialists and made *Ancient Society* the anthropological source book of Marxism. While insisting in *Systems of Consanguinity* that kinship systems are inherited with the blood, Morgan recognized the diffusion of cultural traits as well as the continuity of social development. But this cultural approach remains a minor theme beside the more prevalent one that systems of relationship "have been handed down as transmitted systems, through the channels of the blood, from the earliest ages of man's existence upon earth; but revealing certain definite and progressive changes with the growth of man's experience in the ages of barbarism."[102] Therefore, Morgan had two theories of the rise and development of kinship systems which scarcely came into contact with each other. Deriving "systems of consanguinity" from marital relationships, Morgan on the one hand specifically rejected independent invention, spontaneous growth, and cultural diffusion to explain the universal schemes of counting kin. Instead he made them come down through the blood. Vaguely, and never conflicting with this biological vehicle of cultural transmission, Morgan recognized, on the other hand, the material conditioning of social development. He lacked the clear definition of culture developed by Tylor, a concept which hinged on man's status as "a member of society." With such a concept Morgan might have found both the source of institutions and the means of their perpetuation. In the absence of such a concept, Morgan fell back on biology when he needed an instrument of transmission which would account for the persistence of systems of relationships. Suggesting just such persistence and stability, "blood" would serve well enough.

As previously mentioned, Morgan was influenced by Bastian's concept of *Elementargedanken*, and the way Morgan used "primary ideas" in his study of the evolution of the family illustrates again his fluctuation between an analysis of culture in social and biological terms. Sometimes Morgan argued that social institutions developed from "primary ideas" and moved in a predetermined and logical direction according to their inherent tendencies. In his scheme social evolution was restricted by the inner capacity of germinal ideas to develop along fixed lines. Thus, a system of consanguinity was "under the absolute control, like other domestic institutions, of the primary ideas upon which it is founded. . . . It may be advanced by development from within, in which case it must move in logical accordance with the principles of the system."[103] Like systems of consanguinity, the primary ideas which are their psychological or biological roots were transmitted with the blood stream. Pursuing this line of thought, Morgan claimed that the mixing of diverse racial stocks gradually broadened the logical susceptibilities of primary ideas, permitting a new breadth of social development and the evolution of civilization itself.[104] Although this blending of biological and social categories continued to confuse Morgan when he wrote *Ancient Society*, it had reached its peak in *Systems of Consanguinity* and remained in the background of Morgan's last work as a foreign residue. For one of the great pioneers of the theory of social evolution such a shift in emphasis was inevitable. Persistence in treating social usages and institutions as the predetermined consequences of self-enclosed systems of elementary ideas would have vitiated the main theme of his later work, that the evolution of cul-

[100] Morgan, *Systems of consanguinity*, vi.
[101] White, *Morgan's European journal*, 371.
[102] Morgan, *Systems of consanguinity*, vii.
[103] *Ibid.*, 180. See also pp. 51 and 212.
[104] *Ibid.*, 67.

ture was neither a psychological nor biological, but a cultural process fundamentally conditioned by economic modes of subsistence.

The "logical" development of primary ideas, to which Morgan repeatedly turned in *Systems of Consanguinity*, was a by-product of his evolutionary approach, a fiction of his evolutionary method. The line of evolution of the family once reconstructed, it was easy to interpret its direction as inherently and inevitably moving where, in fact, it did move for some cultures. What made social evolution "logical" was Morgan's retrospective view. What clinched it was his scarcely concealed hypothesis that, given two stages of the evolution of the family, a third and still earlier stage may be supposed to continue the trend evident from a comparison of the latter two stages. This, indeed, would have made cultural development a rational process. Asserting that "It is impossible to develop from the primary ideas . . . a series of institutions which do not spring logically from them" [105] was then only making explicit a fundamental premise of his anthropology. Although Morgan never freed himself from his preconceptions about "primary ideas" and the "blood stream," he could have dispensed with them both. At the same time he could have maintained substantially intact his theory of the evolution of the family by further developing the socio-economic side of his analysis. For paralleling Morgan's interpretation of domestic institutions on the basis of the psychological apparatus of Bastian's *Elementargedanken* is Morgan's theory of the material conditions of society, that a whole organization of society is implied by the dominant economy. Morgan wrote:

> Primitive peoples, having the range of a continent, must of necessity have commenced their career as fishermen, in dependence upon this great primary source of human subsistence, and with but incidental support from the proceeds of the hunt. In the course of time they would learn to domesticate young animals captured in the chase, out of which would come a discovery of the uses of flocks and herds, as a more abundant and a more invigorating means of subsistence. This again, in the lapse of time and through migrations, would be followed by the discovery of cereals, and of the art of cultivation, which would lead inevitably to village life, out of which would spring the first germs of civilization. In addition to this known sequence of the means of progress, the stages of which were doubtless separated from each other by centuries and decades of centuries of time, every nation upon a continent had one or more contiguous nations between whom and itself there was more or less of intercourse. Amongst contiguous nations there would be a free propagation of arts and inventions, which would tend to the general advancement of society throughout the entire area in which these influences were felt. Nations are apt to share in the more important elements of each other's progress.[106]

Although Morgan never realized it, the very development theory of culture with which his name is most closely associated made the dependence of social evolution on biologically transmitted mental germs superfluous and obsolete.

It is obvious that as early as *Systems of Consanguinity* there was a germinal economic determinism in Morgan's thought. He used terms descriptive of economic life for the progressive stages in an evolving culture. He explicitly suggested that the rise of property-holding and the settlement of lineal succession to estates "might be expected to lead to a more precise discrimination of the several degrees of consanguinity if they were confounded by the previous system." [107] He went further by holding that the growth of the idea of property was synonymous with the rise of civilization.

> It is impossible to overestimate the influence of property upon the civilization of mankind. It was the germ, and is still the evidence, of his progress from barbarism, and the ground of his claim to civilization. In fact governments, institutions, and laws resolve themselves into so many agencies designed for the creation and protection of property.[108]

Nevertheless, Morgan's main emphasis in *Systems of Consanguinity* is upon the "great epochs in the ages of barbarism, and the customs and institutions which move society forward," [109] not on the economic conditions of social development. What Morgan had established to his own satisfaction was that "civilization must be regarded as the fruit, the final reward, of the vast and varied experience of mankind in the barbarous ages" [110] and that there is a natural order of domestic institutions. *Ancient Society (Researches into the Lines of Human Progress from Savagery through Barbarism to Civilization)* is the detailed elaboration of these themes, evolution, progress, and economics. In the interval Morgan contributed two papers to the American Association for the Advancement of Science, significantly titled "Arts of Subsistence" and "Ethnical Periods."

[105] *Ibid.*

[106] *Ibid.*, 448.

[107] *Ibid.*, 14.

[108] *Ibid.*, 492.

[109] White, *Morgan's European journal*, 370.

[110] Morgan, *Systems of consanguinity*, vii.

Morgan began *Ancient Society* with a statement of the principle that Lubbock and Tylor accepted as a necessary postulate of the development theory of culture. "The great antiquity of mankind upon the earth has been conclusively established." [111] Repudiating the Mosaic cosmogony and assuming geological time, Morgan treated culture as a natural and necessary evolution. "The latest investigations respecting the early condition of the human race, are tending to the conclusion that mankind commenced their career at the bottom of the scale and worked their way up from savagery to civilization through the slow accumulations of experimental knowledge." [112] Like all the evolutionary ethnologists, Morgan followed Lyell and Darwin in presuming change to be gradual and not cataclysmic. The accumulation of slow, continuous changes followed the order of observable processes in the present and was characteristic of the evolution of all natural phenomena. The continuity of social life, which the cultural evolutionists insisted upon, was a part of the same natural process which included geology and biology, only the sequence was cultural, from savagery to civilization. For the same reason that Lyell had to reject the extraordinary cataclysms of Cuvier, Morgan and the evolutionary anthropologists rejected the theory of cultural regression of Archbishop Whately and the Duke of Argyll. Geological catacylsms and cultural revelations were on a par in destroying the continuity and uniformity of a consistent naturalistic methodology. Darwin, too, of course, in *The Origin of Species* had maintained the mutability of species against special and cataclysmic creations which tore the continuity of the biological world into independent bits.

Insistence on the order and uniformity of nature was a persistent tenet of the geologists, biologists, and anthropologists of this period, and the conviction of the unity of nature reinforced the unity that the anthropologists saw in cultural evolution. The material with which they dealt, the comparative method, and the evolutionary view lent themselves to this interpretation. Morgan, Lubbock, and Tylor understood culture as the product of communities of men, men who, they all insisted, had begun in a stage of utter savagery. Following up their presumptions, the anthropologists drew cross-cultural comparisons from ethnographical data on different peoples at different periods. This procedure concluded in the reconstruction of a sequence of progressive cultural stages which delineated the unity of cultural development. In Morgan's words:

> As it is undeniable that portions of the human family have existed in a state of savagery, other portions in a state of barbarism, and still other portions in a state of civilization, it seems equally so that these three distinct conditions are connected with each other in a natural as well as necessary sequence of progress.[113]

Morgan's own evolutionary position is clear, and in *Ancient Society* the scope of his evolutionary method is extended far beyond kinship, marriage, and family systems. Thinking of government and techniques of subsistence as well as of domestic institutions, Morgan wrote, "It can now be asserted upon convincing evidence that savagery preceded barbarism in all the tribes of mankind, as barbarism is known to have preceded civilization. The history of the human race is one in source, one in experience, one in progress." [114] A more succinct statement of cultural evolutionary principles could hardly be found, but Morgan did a great deal more than represent the general program of the cultural evolutionists.

On a bronze memorial tablet in Aurora, New York, Morgan is celebrated as the "Discoverer of the Law of Social Progress," and although this is an enthusiastic estimate of Morgan's work, it points to an important fact. It is Morgan's distinction that he made a serious attempt to carry the evolutionary analysis beyond the faith of progress by proposing a comprehensive series of stages to illustrate the sequence of cultural devlopment. He did so tentatively and with reservations, but this presumptuous effort singled Morgan out as the whipping-boy of the critics of the evolutionary anthropologists. Tylor's verdict on grandiose cultural generalizations has been the general judgment of professional anthropologists on Morgan's *Ancient Society*. "In fact, much of the labour spent in investigating the progress and decline of civilization has been mis-spent, in premature attempts to treat them as a whole which is as yet only susceptible of divided study." [115] The temerity with which Morgan organized the evolution of culture in a panoramic display of progress was to offend the judicious Tylor as it later offended the empiricists of the Boas school.

To implement the evolutionary program and

[111] Morgan, *Ancient society*, v.
[112] *Ibid.*, 3.
[113] *Ibid.*
[114] *Ibid.*, v–vi.
[115] Tylor, *Primitive culture* **1**: 32.

the theme of progress, Morgan specified the grades of cultural development and found the vehicle of progress in technological evolution. When Morgan did so, both Tylor and Lubbock were stunned at what they saw. Compressed into one volume, the industrial arts, the forms of government, family systems, and the idea of property were all brought into the evolutionary scheme. Ethnic periods, "each representing a distinct condition of society, and distinguishable by a mode of life peculiar to itself," [116] displayed the ladder of evolution and progress. Like Tylor, Morgan interpreted cultures as "wholes." But the peculiar mark of Morgan's evolutionary paradigm was that each ethnic period implied a way of life characterized by a dominant technology.

> It is probable that the successive arts of subsistence which arose at long intervals will ultimately, from the great influence they must have exercised upon the condition of mankind, afford the most satisfactory bases for these divisions.[117]

The ages of Stone, Bronze, and Iron, which had been methodological devices for grading the tools of primitive man, in Morgan's scheme became seven levels of progress from barbarism to civilization, each level a "culture" and each level exhibiting "a mode of life more or less special and peculiar to itself." [118] Specifically, of course, Morgan's ethnic periods were identified with arts of subsistence and not simply with a stone, bronze, or iron technology, although the Danish archaeologist's Age of Iron became literally an "age" for Morgan, not in the temporal sense so much as in the sociological. Each of the seven was a status in the hierarchy of progress—three of "Savagery," three of "Barbarism," and one, the last, indicating the beginning of "Civilization." "Savagery" and "Barbarism" were broken down into "Lower," "Middle," and "Upper" subdivisions. For instance, the "Status of Civilization" began with the invention of a phonetic alphabet; immediately preceding it was the "Upper Status of Barbarism," the period of the invention of the process of smelting iron ore. As such generalizations go, Morgan's characterization of civilization as the literate machine age is incisive enough.

Both Tylor and Lubbock reviewed Morgan's *Ancient Society* and saw it as an extension of the evolutionary theme of *Systems of Consanguinity* "worked up, with other materials, into his present treatise." Tylor found it lacking in precision and consistency, "a structure of theory wider and heavier than his foundation of fact will bear," and he almost sneered at the scope of the material it covered. Morgan's "scheme aims at no less than to map out in a few bold lines of progress the whole evolution of the family and the nation—from when man was a gregarious mute to when he became a cultured republican." [119] Lubbock called Morgan an Iroquois Indian, which he was by adoption, and treated his ethnic periods as an idle taxonomy, a subject for ridicule. He wrote:

> To return to Mr. Morgan's "statuses." Man got into Upper Savagery with the bow, and left it with the "invention of the art of pottery." When he could make pottery, he was a lower barbarian; when he got as far as bronze, and could domesticate animals and grow cereals, he was a Middle Barbarian; with the manufacture of iron he became an Upper Barbarian, and the introduction of phonetic writing, or—for it is all one to Mr. Morgan—of hieroglyphical writing, saw him "in the status of civilization." [120]

This parody on *Ancient Society* does not put Lubbock "on the side of the angels," but it is uncomfortably close to Lady Constance's remarks on evolution in Disraeli's novel *Tancred*.

> You know, all is development—the principle is perpetually going on. First there was nothing; then there was something; then—I forget the next—I think there were shells; then fishes; then we came—let me see—did we come next? Never mind that; we came at last, and the next change will be something very superior to us, something with wings.[121]

Morgan may have been given a bad time by his British critics, but he was quickly honored in his own country. He was elected to the presidency of the American Association for the Advancement of Science.

Adolf Bandelier, Morgan's close friend and disciple, also reviewed *Ancient Society*, and his comments are particularly instructive. When Bandelier found two theories of cultural evolution, two ways of tracing progress, in the book, he became the first of Morgan's readers to note a dichotomy in his scheme of progress. Morgan

[116] Morgan, *Ancient society*, 8.
[117] *Ibid.*, 9.
[118] *Ibid.*, 13.
[119] Tylor, Edward B., *Ancient society*, *The academy* **14**: 67, 1878.
[120] Lubbock, John, *Ancient society*, *The Saturday review of politics, literature, science, and art* **45**: 20, January 5, 1878.
[121] As quoted in Stow Persons' *Evolutionary thought in America*, 149, New Haven, Yale University Press, 1950.

was never completely clear about its interpretation himself and explained at the beginning of *Ancient Society:*

> An attempt will be made in the following pages to bring forward additional evidence of the rudeness of the early condition of mankind, of the gradual evolution of their mental and moral powers through experience, and of their protracted struggle with opposing obstacles while winning their way to civilization. It will be drawn, in part, from the great sequence of inventions and discoveries which stretches along the entire pathway of human progress; but chiefly from domestic institutions, which express the growth of certain ideas and passions.
>
> Two independent lines of investigation thus invite our attention. The one leads through inventions and discoveries, and the other through primary institutions.[122]

To Bandelier these two ways of investigation appeared as mutually exclusive alternatives, and he indicated in his review a preference for the line of evolution leading through institutions and dismissed the other, the technological, without serious discussion. Marx and Engels took the opposite tack, and *Ancient Society* became the main anthropological support of Engels' *The Origin of the Family, Private Property, and the State;* for, in the words of Engels, Morgan had "in a manner, discovered anew the materialistic conception of history, originated by Marx forty years ago."[123] When Morgan used this line of evolution, which leads through inventions and discoveries or the arts of subsistence, he constructed a paradigm of progress in ethnic periods.

Despite all the disagreements which it provoked, no one has contested the fact that *Ancient Society* is an evolutionary book. Each of its four parts has a title introduced by the word "Growth," whether "The Growth of Intelligence Through Inventions and Discoveries" or "The Growth of the Idea of Government." Although Morgan divided his material into four evolutionary, "parallel lines along the pathways of human progress,"[124] he discovered that one of the four was so important that it might become the general denominator of progress and the basis for discriminating the ethnical periods from barbarism to civilization. This was the evolution of the arts of subsistence, and the remaining three are best understood as comprising what Morgan thought of as "primary institutions," since it is to the family and government that the body of the rest of *Ancient Society* is devoted. The relation between these fundamental divisions defines the problem. For on the one hand Morgan himself insisted upon the distinction between the evolution of arts of subsistence and the evolution of institutions; yet on the other hand he went a long way toward making technology the dynamic force in the evolution of institutions. In good part the issue is to keep straight what Morgan said and what Morgan did, for if one follows the details of his treatment of institutional development, the hard-and-fast distinction with which he began *Ancient Society* becomes blurred.

As Morgan's earlier work indicates, his preparation for handling the evolution of government (*The League of the Iroquois*) and the evolution of the family (*Systems of Consanguinity*) was far more adequate than for the writing of a systematic treatise on the economic determination of culture. Morgan's task was made more difficult by an archaic psychology in the midst of his cultural analysis, a psychology which *Ancient Society* itself finally challenges. For in the development of social organizations which Morgan minutely described, primary institutions were actually not made dependent upon primary ideas, but, like the industrial arts, evolved from what had gone before, dependent upon the material context. The "idea of government" and the "idea of the family" did not constrain social evolution; Morgan made them creatures of cultural evolution expressive of deep-seated human needs. As indicated earlier, the adjective "logical," which Morgan applied to the evolution of social institutions, was the term likely for one who saw that what had been will be and that evolution was progress. Beginning with monogamy and the liberal political institutions of modern England and America, Morgan traced the development of the family and the state through a series of stages increasingly remote from modern institutions. The welter of technological arts, inventions, and tools revealed something of the same progressive trend, accelerated by the tendency to multiply which Morgan formalized in the "ratio of human progress."

Of course, all of culture, whether technological or institutional, was an evolution for Morgan. But while Morgan traced the family and government from "a few primary germs of thought" evolving in a "logical" order, he simply made inventions and discoveries the secular outgrowth of the accumulated experience of mankind. The crass and tangible quality of artifacts and the skill

[122] Morgan, *Ancient society*, 4.

[123] Engels, Frederick, *The origin of the family, private property, and the state*, 9. Chicago, Charles H. Kerr, 1902.

[124] Morgan, *Ancient society*, vii.

of artisans encouraged no such special handling as the "ideas," "passions," and "aspirations" with which Morgan associated institutions. Morgan identified institutions, as did Malinowski later, with the basic needs of the organism. He linked every permanent institution of mankind with a "perpetual want," and because the want derived from the organism, Morgan thought of it as primary and given. In the manner of the evolutionary anthropologists, he was looking for antecedents and tracing them to a source unimpeachably natural; but having found the ultimate origin of institutions in primary ideas, he was committed to a psychological as well as a cultural interpretation in describing non-technological evolution. It was not the growth of government or property or the family with which Morgan was exclusively concerned in *Ancient Society*. Rather his attention was turned to the "Idea of Government," the "Idea of the Family," and the "Idea of Property." Thus, because institutions, in contrast with tools and techniques, peculiarly lent themselves to a psychological interpretation in Morgan's system, the distinction between arts of subsistence and institutions became accentuated. Without ever making institutions independent of their social context, Morgan, therefore, paralleled his analysis in cultural terms with another approach and focused on the "logical" development of germs of thought. The institution of the family, for instance, was not only what Morgan would have called a "great power and passion of the mind" but a "creature of the social system, and will reflect its culture."[125]

Then, by invoking the principle of the similarity of the human mind, "a common principle of intelligence . . . in the savage, in the barbarian, and in civilized man,"[126] Morgan found an additional reason for the universal pattern of cultural evolution and a new argument for the unity of man's origin. In the concluding pages of *Ancient Society* Morgan struck a final blow against the degeneracy theory of savages.

> It is the hypothesis of human degradation to explain the existence of barbarians and of savages, who were found, physically and mentally, too far below the conceived standard of a supposed original man. It was never a scientific proposition supported by facts. It is refuted by the connected series of inventions and discoveries, by the progressive development of the social system, and by the successive forms of the family.[127]

[125] *Ibid.*, 491.
[126] *Ibid.*, 553.
[127] *Ibid.*, 506.

9. CONCLUSION

The comparative method, the concept of cumulative experience, the idea of progress, and the cosmopolitan range of nineteenth-century ethnology almost seem to duplicate the philosophical program of the Enlightenment. Were it not for the scientific discoveries and theories of an intervening century, the suggestion would immediately arise that Tylor, Lubbock, and Morgan are direct intellectual descendants of Condorcet, Herder, Turgot, and D'Alembert. Although they shared a community of interests and ideas with social philosophers of the preceding period, the evolutionists' optimism concerning human advancement did not come to rest on reason as the vehicle of progress. Nor was their optimism expressed by a confident belief in the perfectability of man. Their estimate of human nature was a commonplace observation of the ways of man, and as the nature of their general enterprise dictated, man, more adequately described as a higher primate than a fallen angel, was neither exalted nor depraved.

The complexion of human nature was neutral; it was malleable enough to permit a wide liberality and diversity in the features of a social system. Man was not a rational but a utilitarian animal who knew an improved technique when he used one, and having used it, was unlikely to abandon it for a cruder prototype. ". . . No one would go back from letter-writing to the use of the quippu or hieroglyphics; no one would abandon the fire-drill and obtain fire by hand friction."[128] By this reasoning Lubbock and Tylor approached a definitive psychology only so far as to endow man with a little industry and to refuse to make him blind to his own advantage. The continuity of culture offered the means of modifying older tools and techniques into improved ones, and when such advances were made, they were likely to become permanent features of a cultural heritage. Degradation in such circumstances would be folly of a perverse sort; the common sense of utility, a

[128] Lubbock, *The origin of civilisation*, 341. Tylor takes the same position, writing, ". . . The adaptation of a cord to the fire-drill is obviously an improvement on the simpler instrument twirled by hand, and the use of the spindle for making thread is an improvement on the clumsier art of hand twisting; but to reverse this position, and suppose the hand-drill to have come into use by leaving off the use of the cord of the cord-drill, or that people who knew the use of the spindle left it off and painfully twisted their thread by hand, is absurd." *Primitive culture* **1**: 63. See also Tylor, *Researches*, 185 and 193.

practical turn of mind, protected the race against it.

The evolutionists were content to leave progress to the process of building on "to what lies at hand," and, having assumed the immemorial stretch of time covered in cultural evolution, they found no reason to put a large burden on human reason and ingenuity. Both Tylor and Morgan contended that "in the lower ranges of culture progress would be extremely slow in comparison with that which experience shows among nations already far advanced."[129] Taking his clue from the rapidity of social change in modern times and noting the sluggishness of the first stages of social evolution, Morgan attempted to formulate a law of the ratio of human progress. Cultural accumulation, although at first necessarily slow, would in time become a significant growth. The locus of progress was cultural evolution itself, the evolution of the historical community, not the heroic and inventive genius of those men whom Tylor described as "transmitters and modifiers of the results of long past ages."[130]

The animus that made the evolutionists, whether biologists or ethnologists, spokesman for a common point of view was a firm conviction about human history which was at the same time a scientific methodology. By their insistence on the genetic method, they intended to reject mythological explanations for tame ones. The evolutionary view was an analytical tool, and like any analytical tool, this one determined the kind of question asked and, in turn, the kind of answer pertinent to that question. Posing the question of origin, evolutionary studies furnished ready answers in terms bound to impress a generation self-consciously scientific: connecting episodes and transitional stages relieved the stark, dumb quality of the present. They gave civilization as modern man knew it a legitimate pedigree which even the British Association for the Advancement of Science would honor. After all, connections were causal, and the scheme of evolutionary development was not a random, lawless thing, but lent itself to generalization and orderly sequence.

The clue concept in the evolutionary approach was continuity, a continuity depressingly slow in displaying effects but unmistakably real. Piecemeal accumulation, the Darwinian theme, was dominant. But another theme with no Darwinian analogy emerged from the ethnologists' studies of culture: the persistent holding on with all the force of ingrained habit, of practices hedged about with myth and taboo. The staying power of custom, preserving the established ways sanctified by the ancestors, was a new form of heredity just as real as the heredity of bodily structure. This cultural legacy fastened on a new generation a way of life not of its own making, and whether a product of independent development or a gift from further abroad, there was still its history or evolution to be traced.

Tylor's precise concept of the subject matter of ethnology was implicit in the drive to make the study of culture a naturalistic discipline. The idea that culture was a social, group product and not the labor of tongue-tied hermits, made the human creation of culture credible to minds attuned to matters of fact. Credulity might be strained at the thought that civilization was a free gift from on high, but there was nothing to balk the imagination in the view that civilization was the bounty of an historical community capable of cumulative development. If the social life of man was no exception to a universal order of nature, then culture was not a creation *de novo*. The works of man all had a past, and what now was had grown out of what had gone before. The genesis, growth, and diffusion of those works made up the compelling problems of cultural anthropologists; and, if there was to be a science of culture, the search for cultural origins in the continuity of social history became imperative.

On the presumptions drawn from current paleontology, archaeology, biology, and geology Tylor, Lubbock, and Morgan joined evolution and culture and made the development of culture a natural process embracing the whole family of man. They found hope for continuing advancement in a cultural evolutionary process in which racial distinctions might be dismissed as secondary and unimportant. The evolutionists tagged the simplicity and beauty of savage life as no less a myth than a primeval Golden Age. To them it seemed ridiculous to believe that the life of early man was relieved from squalor and brutishness by an unequaled and compensating freedom.

> There are, indeed, many who doubt whether happiness is increased by civilization, and who talk of the free and noble savage. But the true savage is neither free nor noble; he is a slave to his own wants, his own passions; imperfectly protected from the weather, he suffers from the cold by night and the heat of the sun by day; ignorant of agriculture, living by the chase, and improvident in success, hunger always

[129] Tylor, *Primitive culture* **1**: 55.
[130] *Ibid.*, 17.

stares him in the face, and often drives him to the dreadful alternative of cannibalism or death.[131]

The life of primitive man in a state of nature was all that Hobbes had said it was, except perhaps, never knowing a pre-social condition, man had never enjoyed that precarious freedom of being without law or master. It was not "dawn man," or the free and noble savage, or a Golden Age which stirred the imagination of the evolutionists and lured them to retrieve the past; it was progress and progress beckoned to the future.

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[131] Lubbock, *Pre-historic times*, 569.

THE IMPACT OF UNIFORMITARIANISM

Two Letters from John Herschel to Charles Lyell, 1836–1837

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I. INTRODUCTION

CHARLES DARWIN began the Introduction to *On the Origin of Species* by saying that his observations in South America "seemed to throw some light on the origin of species—that mystery of mysteries, as it has been called by one of our greatest philosophers." The philosopher was the famous astronomer John Herschel, and the phrase, "that mystery of mysteries," came from Herschel's monumental letter of February 20, 1836, written from the Cape Colony (where Herschel was engaged in surveying the southern heavens) to his friend Charles Lyell. The letter is here published in full for the first time, from the manuscript in the Darwin-Lyell collection of the American Philosophical Society, together with the surviving part of Herschel's follow-up letter of June 12, 1837.[1]

Herschel's 1836 letter is a major document in the scientific controversies of the period. Lyell's system of geology ("Uniformitarianism") as expounded in his *Principles of Geology* of 1830–1833 had challenged contemporary British geological thought, which was still influenced (although not dominated) by the ideas of George Cuvier and Cuvier's more impetuous English disciple William Buckland.[2] In addition, Lyell's system had also implicitly raised the question of how new species are generated. Lyell himself was too fearful of orthodox Christian opinion to assert a naturalistic origin of species in the *Principles*, especially as he could specify no mechanism whereby such generation could take place. Indeed, large parts of his second volume were devoted to a refutation of the evolutionary ideas of Lamarck, and this refutation became standard for the period. It is easy to see why Lyell wished to rebuff Lamarck. An evolutionary biology would imply an evolutionary geology as well, and Uniformitarianism pictured the world not as having progressed from some initial chaos to its present condition, but as having gone through an indefinite number of essentially repetitive stages. Lyell's opponents, who were named by one of their members the "Catastrophists," were quick to point out Lyell's omission and rely upon it in constructing a logical case for the supernatural nature of species creation.[3]

It was John Herschel, and not Lyell, who spoke out in favor of "a natural in contradistinction to a miraculous process" of species creation, and this part of his letter was made public in 1837 in an appendix to Charles Babbage's *Ninth Bridgewater Treatise*. To appreciate the importance of this testimony it is necessary to recall John Herschel's enormous prestige in the 1830's and 1840's. As the son of the great William Herschel and as a brilliant astronomer in his own right, he was not merely the most famous scientist in England: he was looked up to very much as though he were a god. The Duke of Sussex was only summing up the common opinion when in his presidential address to the Royal Society in 1833 he said that Herschel was "such a model of an accomplished philosopher as can rarely be found beyond the regions of fiction."[4] Sheltered by Herschel's reputation, then, other

[1] I wish to thank the staffs of the Library of the American Philosophical Society and of the Map Room of the Widener Library, Harvard University, for their interested as well as efficient assistance.

[2] The ideas of Buckland were challenged most heartily by John Fleming; see esp. The geological deluge, as interpreted by Baron Cuvier and Professor Buckland, inconsistent with the testimony of Moses and the phenomena of nature, *Edin. Philos. Jour.* **14**: 205–239, 1825–1826. Cuvier's English translator argued with his author in notes: Cuvier, Georges, *Essay on the theory of the earth*, tr. Robert Jameson, 5th ed., 334, 429, 436, Edinburgh and London, 1827. The *Quarterly Review* began criticizing Buckland as early as 1826, in Transactions of the Geological Society, *Quarterly Review* **34**: 517–518, 1826 (the article was by young Charles Lyell). Equally important is the fact that a number of geologists did not care very much for theoretical matters of any kind.

[3] Whewell, William, Lyell's Geology, vol. 2, *Quarterly Review* **47**: 126, 1832. For Catastrophist theory in general see my article, The problem of miracles in the 1830's, *Victorian Studies* **4**: 5–32, 1960.

[4] Augustus Frederick, Duke of Sussex, Presidential address of 1833, *Phil. Trans. Abstracts* **3**: 224, 1830–1837.

scientists could consider the problem of the origin of species on its merits, without fear of religious orthodoxy. Since Herschel was known to be a sincere Christian, only extremists could henceforth claim that scientists should submit their findings to an Evangelical interpretation of the Bible. There were such extremists, of course, but they had no influence on the course of scientific debate. Actually Lyell's ideas had been received much more graciously than he had expected, for the Christians among the scientists were just as eager as was Lyell to keep their science from being subjected to undue influence from scientifically illiterate Bibliolaters. It was indeed such strategically placed Christians as Adam Sedgwick at Cambridge, William Buckland at Oxford, and William Vernon Harcourt at York who did combat most vigorously with Bibliolatry. Yet Lyell remained uneasy, and agreed only reluctantly to let Babbage print Herschel's assertion. The still more explosive part of Herschel's letter, in which he suggests 50,000 years apiece as the age of the Patriarchs, thousands of millions of years for each of the Days of Creation, and a view of Biblical miracles which makes them essentially subjective experiences,[5] never did get into print.

As young Charles Darwin developed scientifically in the sheltered arena of the Geological Society of London, the matter is of some importance in interpreting his development. Darwin was able to be almost completely insensitive to theological considerations concerning the origin of species, so much so that he did not even understand what the phrase "the creation of species" meant to the people he criticized. In his 1842 sketch he presented the crude notion that a "creationist" must believe that the individual species of rhinoceros have arbitrarily come together from the dust.[6] This notion bears no relation to the carefully rationalized explanation of such a Catastrophist as, say, William Whewell. The orthodoxy Darwin was hesitant to break from —"it is like confessing a murder," he said[7]—was the Uniformitarianism of his friend and mentor Charles Lyell with its strongly anti-developmental bias. Darwin never considered, nor had to consider, whether or not a naturalistic explanation of the origin of species would even be admissible. That problem was settled, for competent intellectual circles, by John Herschel's expression of belief in 1836.

Important as the first paragraph of Herschel's letter was at the time, the full text is of still greater interest of the historian. First, it is the best document available to demonstrate the intellectual impact of Lyell's *Principles*. Herschel does not merely *say* that he is impressed; he demonstrates in page after page of suggestions, queries, observations, and theorizing how thoroughly the most distinguished scientific mind of the period has been stimulated by Lyell's arguments. The busy astronomer has found time to read a twelve-hundred page book *three times!* "and every time with increased interest."[8]

Second, Herschel's admiration of Lyell's approach—extending even to a belief that it should be applied in other sciences[9]—was of considerable importance in the strictly geological debates of the period. Lyell's Uniformitarian system was by no means readily accepted by his fellow geologists, even when the particular contents of his volumes were admired, as they almost universally were. Indeed Uniformitarianism never became the dominant geological school in England all the way down to 1859, when both Uniformitarianism and Catastrophism as distinct schools were swallowed up in the new evolutionary approach. But while the testimony of young Charles Darwin gave Lyell expert field support, the adherence of John Herschel meant that the Catastrophist opposition could not solidify as a mathematically-oriented Cambridge group, as might otherwise have happened. To understand the situation, it is necessary to know the personal relations of the principal characters. John Herschel, Charles Babbage, William Whewell, and George Peacock had been Cambridge contemporaries who had cooperated in converting Cambridge from Newton's fluxions to the new "French" methods of notation in the calculus. Adam Sedgwick as Professor of Geology later aligned himself with this group. Roderick Murchison, Lyell's traveling companion on a fruitful geological tour of Europe in 1827, came under the influence of Sedgwick during their joint endeavors to untangle the Silurian and

[5] Below, section II, paragraph 14.

[6] Darwin, Charles, Sketch of 1842, *Evolution and natural selection*, ed. Gavin de Beer, 83-84, Cambridge, Cambridge University Press, 1958.

[7] Darwin, Charles, *Life and letters* 1: 437, New York, 1887.

[8] Below, section II, paragraph 1. The edition Herschel refers to in his letter actually has about 1700 small pages; the second edition, with a larger page size, had run to over 1200 pages.

[9] Below, section II, paragraph 14.

Cambrian strata in the early 1830's. Sedgwick and Murchison were the best field geologists of the period, and Whewell was a more agile philosophizer than was Lyell. With first Sedgwick, then Whewell, then Murchison acting as the intellectual leaders of the Catastrophist attack on Uniformitarianism, and with the later adherence of the mathematician William Hopkins and the Scots physicist James David Forbes, there was clearly the possibility that Lyell's position would be read out of court as simply "not scientific enough." That this attempt did not develop successfully was due in part to the fact that John Herschel—the man to whom they all looked up, and whose book on scientific methodology was a classic for the period[10]—came out strongly for Lyell. Since in addition the mathematician Charles Babbage was also inspired by Lyell's work, the Catastrophists could only argue that their case was better than was Lyell's, and this running debate led indirectly to the Whewell-Mill controversy concerning the philosophy of science in the 1840's. Herschel himself never decisively entered this later debate, although he was clearly uneasy about the tendencies of the usual brand of British empiricism.[11] He did, however, attempt to suggest that the Uniformitarian-Catastrophist disagreement was merely one of degree and should not be taken so seriously by either group, both of which contained his personal friends.[12] In this judgment he was, of course, incorrect, since the Uniformitarian antidevelopmental view of the world was quite antithetic to the Catastrophist insistence on a world developing geologically from a primitive chaos.

What the adherence of Herschel and Babbage to Uniformitarianism shows the modern historian, however, is that Lyell, without consciously intending it, had produced a geological system based on such nicely balanced forces ("aqueous" *vs.* "igneous" forces) that it had abstract properties similar to those of physical astronomy. Herschel makes no secret in his letter of the relish with which he has tackled the problem of reducing the unruly forces of the volcano and the earthquake to a slow basic interaction between the heat of the sun and the internal heat of the earth —as simple a pair of forces as an astronomer could desire.[13] And a system, we may note, which he uses to offset ideas of development in a single direction.

Finally, Herschel's letters are of great interest to the student of Herschel. Herschel published monographs or books on mathematics, optics, meteorology, physical geography, chemistry, "galvanism," photography, scientific methodology, and of course astronomy. Here we see him also elaborating a large-scale geological theory, growing plants, studying species, discussing geographical distribution, commenting on linguistics, measuring the germination time of seeds, observing granite dikes and petrified shrubs, explaining the geological history of the Cape, and collecting specimens for his friends. All this not in a period of leisure but when he was doing nothing less than to chart every nebula in the southern heavens! We can begin to appreciate Herschel's reputation in his own time, and to see the outline of a man of great ability and a bewildering variety of interests who was never able to do just that one thing which would keep his name thoroughly alive in the history of science. The impression of mental scope is increased by a perusal of the manuscripts themselves. Clearly they were written as letters, not as formal treatises for publication. Herschel was writing so fast that he ignored punctuation and sometimes spelling, could not be bothered to write out "and," wrote "dynamic" at one point and had to correct it to "static" (a winner of the Copley medal!), misread the lettering on one of his own sketches, and formed his letters so crudely that at one point even he forgot whether he had written an "i" or an "e." Yet the result is a production which was in its main theory perfectly adequate for presentation to the Geological Society of London.

A word should be said about this theory of "isothermal surfaces" presented at length in the 1836 letter. It was this which stimulated Charles Babbage's publication of parts of the letter, since, as the correspondence indicates, Babbage had presented a similar scheme in his paper on the Temple of Serapis.[14] Babbage undoubtedly has the pri-

[10] *Preliminary discourse on the study of natural philosophy*, London, 1830.

[11] *Cf.* my article, John Herschel and the idea of science, *Journal of the History of Ideas* **22**: 222, 226–227.

[12] Herschel, John, Whewell on inductive sciences, *Quarterly Review* **68**: 201, 1841.

[13] Below, section II, paragraph *26*.

[14] Babbage, Charles, Observations on the temple of Serapis, *Proc. Geol. Soc. London* **2**: 72–76, 1833–1838; read on March 12, 1834, and printed in No. 36 of the *Proceedings*. This is only a brief abstract; the theoretical part is expanded in his *Ninth Bridgewater Treatise*, 187–201, London, 1837; and the full paper is given in *Quar. Jour. Geol. Soc. London* **3**: 186–217, 1847.

ority but his suggestions were worked out in less detail at the time. However, Lyell did not incorporate the suggestions of either man into his future editions. Such grandiose speculations were always what Lyell feared, especially ones based on the laws of physics. Lyell did not understand physics, but he knew enough history to know how little such extrapolations from supposedly general physical principles had done for geology, from the days of the seventeenth-century physico-theologians to his own day. He was also very skeptical about any arguments based on the internal heat of the earth. It was rather the Catastrophists, especially William Hopkins in the 1840's and William Thomson in the 1850's, who appealed to general physical principles in order to refute Uniformitarianism.[15] Since the ideas of Herschel and Babbage were, nevertheless, quite Uniformitarian in character, they did not appeal to the Catastrophists either, and hence had no important effect on the course of geological debate.

In editing the letters I have tried to reproduce them as naturalistically as possible, with all of the abbreviations, omissions, false starts, and corrections which are characteristic of the manuscripts. There are enough divergences from normal usage in the originals so that it would not be practicable to indicate them by [*sic*]; the reader may assume that any such divergences were put there by Herschel. Words crossed out in the original are here enclosed in brackets | |. Words added to the original above the line or obviously as an afterthought are in bold-face type. Herschel had several footnotes in the letters; these are indicated by an asterisk * and I have signed them with his initials: JFWH. The other footnotes are mine. Herschel often ended a sentence by bearing down heavily on the final tail of, say, the "t" in "result." There is no way to reproduce this effect in print and I have in such cases credited him with a period. Two stylistic points of interest are: (1) Herschel customarily abbreviates "and" by a small script alpha, not by & (as used here), which he uses only rarely; (2.) Herschel says "in page 25" rather than "on page 25."

The manuscripts themselves are in good condition except in two places where the creases have worn through completely. However, Herschel had a reasonably difficult hand, and one point in particular should be noted. There is simply no way to tell visually if an initial letter is intended to be a capital, especially if it is an *s*, *c*, *v*, *w*, *c*, or *b*. After working over the letters it seems clear to me that Herschel himself had no clear notion of when he wished to capitalize a word other than at the beginning of a sentence, but that he, nevertheless, did so wish now and then. I have therefore relied on my own judgment in a number of cases, and another editor might arrive at different results. Of course capitalization had no ideological importance in the 1830's; for example, most writers normally did not capitalize a pronoun such as "his" when referring to God.

The 1836 letter is on grayish paper, a full sheet of which measures 7 15/16" by 9 13/16". The sheet is folded in the middle and turned sideways so as to make four pages each about 5" wide and 8" tall; and the entire letter consists of three such sheets, or twelve pages. Thus the size of Herschel's handwriting is fairly small, as these twelve pages transcribe into twenty double-spaced typewritten pages of the normal size.

I have numbered the paragraphs of each letter for easy reference. Parts of the 1836 letter have been published, as follows: a brief abstract of parts of paragraphs 8, 9, 10, 11, 12, 25, and 26 constitute Herschel's article, On secular variations of the isothermal surfaces of the earth's crust, *Proc. Geol. Soc. London* **2**: 548–550, 1833–1838, read on May 17, 1837. Paragraphs 1, 8, 9, 10, 11, 12, 13, 25 (incomplete), and 26 (incomplete) were published by Charles Babbage in Note I, pp. 202–213, of his *Ninth Bridgewater Treatise*, London, 1837. Both of these selections were cleaned up for publication. Babbage, for instance, corrected the spelling, substituted heavy Victorian punctuation for Herschel's intuitive dashes, spelled out abbreviations, and occasionally left out a phrase—in all, between thirty and fifty "corrections" per page of manuscript. The result is a letter which looks as though it had been composed as a formal treatise. Babbage reproduced somewhat less than 45 per cent of the total letter and, except for the first paragraph on the origin of species, confined his reprint to the theory of isothermal surfaces.

II. THE 1836 LETTER

Feldhausen. C. G. H.
Feb. 20. *1836.*

My dear Sir.

1. I am perfectly ashamed not to have long since acknowledged your present of the new Ed. of your

[15] See my article, The Uniformitarian-Catastrophist debate, *Isis* **51**: 38–55, 1960.

Geology[16] a work which I now read for the 3d time and every time with increased interest, as it appears to me one of those productions which work a complete revolution in their subject by altering entirely the point of view in which it must thenceforward be contemplated. You have succeeded too in adding dignity to a subject already grand by exposing to view the immense extent & complication of the problems it offers for solution and by unveiling a dim glimpse of a region of speculation connected with it where it seems impossible to venture without experiencing some degree of that mysterious awe which the Sybil appeals to in the bosom of Aeneas on entering the confines of the shades—or what the maid of Avenel suggests to Halbert Glendinning

He that on such quest would go must know nor fear nor failing

To coward soul or faithless heart the search were unavailing—

Of course I allude to that mystery of mysteries the replacement of extinct species by others. Many will doubtless think your speculations too bold—but it is as well to face the difficulty at once. For my own part—I cannot but think it an inadequate conception of the Creator, to assume it as granted that his combinations are exhausted upon any one of the theatres of their former exercise—though in this, as in all his other works we are led by all analogy to suppose that he operates through a series of intermediate causes & that in consequence, the origination of fresh species, could it ever come under our cognizance would be found to be a natural in contradistinction to a miraculous process—although we perceive no indications of any process actually in progress which is likely to issue in such a result.

2. Speaking of the destruction of species there is here a very lovely species **of plant** w[h] seems verging rapidly to extinction—the Disa Grandiflora. It grows only on the summit of the Table Mountain, and (as I am told) on no other Mount[n] in the Colony & there it is already become difficult to find as every one who ascends brings away a root with a view to cultivate it below, an attempt which only succeeds with extreme care, owing to the utter diversity of atmospheric circumstances at the top of the M[n] & below. I have two specimens in my garden where they have been lingering a year & a half, but I have no hope of their surviving.

3. This is a beautiful country for studying the graduation of Botanical species—the families are so rich in species. I am little or nothing of a Botanist—but with one feature it is impossible not to be struck—viz that when you find a species which fills up as you fancy a wanting link between two others—it does not *merely* fill it, but does so with the superaddition of some new characters—or some analogy with a 3d species which the others do not offer.

4. I am not sure that I am duly impressed with the argument for the originally limited distribution of particular Botanical groupes in particular Regions.[17] At least it seems that the argument may be inverted & that it may be contended that any given groupe, observed to be confined to a particular district is in fact only the last surviving remnant of the same groupe universally disseminated, but in course of extinction—nor do I see how to distinguish, supposing only one individual of a given species existed in the world—whether that species were just nascent—or just dying out. Perhaps both processes are going on at once—some groupes may be spreading from their foci others retreating to their last strong holds.

5. Your solution of the great Problem of the variation of Climate appears to me perfectly satisfactory.[18] It is a bit like tracing out the course of the Niger in African geography or like Landen's theorem in the rectification of the Conic Sections—a great & acknowledged difficulty fairly surmounted.

6. Will you excuse my remarking, on your names Pliocene & Miocene I do not think your reason for omitting the e a good one.[19] The continental reader will call your words Pl*e*cos*a*ne & M*e*cos*a*ne. In English ei is often pronounced like long i* and in German they have no other representative of that sound. By omitting the e the genius of our pronunciation *is* consulted it is true, but in its most objectionable feature—nay you have even introduced an ambiguity, for some readers seeing these words have a foreign look will frenchify them (as indeed I have heard it done) & pronounce them just as you desire to avoid.—In all other respects Pleiocene Meiocene & Eocene are excellent.

7. What think you of Beke's speculations about the silting up of the Euphrates.[20] Is there geological

[16] Herschel's citations in this letter are to the fourth edition of Lyell's *Principles of geology*, London, 1835, as are my footnotes unless otherwise noted.

[17] In *Principles* **3**: chapter 8.

[18] In *Principles* **1**: 175–193. Lyell's much-discussed theory of climate change was not included as one of the grounds for the award of the Royal Society's Royal Medal in 1834; the "Report of the Council" excluded all "controverted questions" in the *Principles* from the award: *Phil. Trans. Abstracts* **3**: 306, 1830–1837. Perhaps Lyell was a bit annoyed at this; if so, it would explain his extravagant comments in his reply to Herschel (below, section III). The theory of climate change *was* specifically included in the award of the Copley Medal in 1858: *Proc. Roy. Soc.* **9**: 513, 1857–1859.

[19] Like many of his contemporaries Lyell had William Whewell supply him with scientific nomenclature, in this case for the four new divisions of the Tertiary [Cenozoic] era which Lyell proposed. Whewell proposed *æcene* as the fourth term but Lyell preferred to work with *eocene*, *miocene*, and older and newer *pliocene*: Todhunter, Isaac, *William Whewell* **2**: 108–111, London, 1876. I don't know just when pleistocene was introduced.

* As in "sleight" (of hand).—height—&c—JFWH.

[20] Beke, Charles T., *Origines biblicae*, 19–21, London, 1834. Beke, the famous Abyssinian explorer of the 1840's, was earlier a law student, as Lyell had been. His *Origines* was an attempt to establish early history on the premise of the historical accuracy of the early books of the Old Testament. In his preface he assured his readers that he had avoided reading modern German scholarship lest he be unduly influenced. In the section referred to he was worried that one of Lyell's phrases might be interpreted to mean that the Euphrates had joined the Tigris in geological times before the advent of man.

evidence? As to his Historical evidence, it seems to me "clear as mud" that the matter is got betuzzled by the different stadia referred to, & that the puzzle is inextricable.

8. Now for a bit of theory. Has it ever occurred to you to speculate on the probable effect of the transfer of pressure from one part to another of the Earths surface by the degradation of existing and the formation of new continents—on the fluid or semifluid matter beneath the outer crust? Supposing the whole to float on a sea of Lava, the effect would merely be an almost infinitely minute flexure of the strata—but supposing the layer next below the crust to be partly solid & partly fluid & composed of a mixture of fixed Rock, liquid lava, and other masses in various degrees of viscosity and mobility, great inequalities may subsist in the distribution of pressure and the consequence may be local disruptions of the crust where weakest and escape to the surface of lava &c.—If the obstructions to free communication among distant parts of a fluid be great no instantaneous propagation of pressure can subsist—the Hydro [dynami] **statical** law of the equality of pressure being only true of fluids in a state of undisturbed equilibrium.—If the whole contents of the fissures, pipes, &c into which we may consider the interior divided, were lava, it is true no increase of pressure on the bed of an ocean from deposited matter could force the lava up to a higher level than the surface or so high —lava being heavier than mud or water. But if the contents be partly liquid, partly gaseous, or partly water in a state to become steam at a diminished pressure—then it may happen that the joint specific gravity of Lava + gas or Lava + steam occupying any given channel may be less than that of water—or of the joint column of water & newly deposited matter which may be brought to press upon it by any sudden giving way of support—and the effect will be the escape of a mixture of lava and gas, either together as froth & pumice, or by fits according as they are disposed in the Channel. This (taken as a general cause of volcano's) would account for the great quantity of gaseous matter which always accompanies eruptions & for the final *blow out* of wind & dirt with which they so often terminate. It has always been my greatest difficulty in geology to find a *primum mobile* for the Volcano, taken as a general, not a local phenomenon. Davey's speculations about the oxidation of the Alkaline metals seems to me a mere chemical dream & the fermentation of water & pyrites as utterly insignificant on a scale of any magnitude.[21] Poulett Scropes notion of solid rocks flashing out into lava & vapour on removal of pressure[22] [sets out] and your statement of the probable cause of volcanic eruptions in p. 385 Vol. 2. when you speak of the effect of a minute hole bored in a tube in which liquefied gasses are imprisoned, both appear to me wanting in explicitness—and as not going high enough in the enquiry, up to its true beginning—and also as giving in some respects a wrong notion of the actual nature of the process itself.—The question stares us in the face—How came the gases to be so condensed—Why did they submit to be urged into liquefaction—If they [be merely the] were not originally elastic but have become so by subterranean heat —Whence came the heat? and *Why did it come?* —How came the pressure to be removed or what caused the crack? &c &c.

9. It seems clear that if the gases or aqueous vapour were once free, at so high a degree of elasticity as is presumed, there exists no adequate cause for their confinement—the spring once uncoiled, there is nowhere a power capable of bending it up to the pitch. We are forced therefore to admit that the elastic force has been superadded to them during their sojourn below by an accession of temperature. Now though I cannot agree with you in your view of the subject of the Central heat (page 373. 5^{1815}) (because I see no reason why the heat may not go on increasing *to the very center* without necessitating such disturbance of equilibrium as to give rise to any circulation of currents which you there seem to regard as the necessary consequence of such a state*) yet I agree entirely with what you observe in p. 376 —that the ordinary repose of the surface argues a wonderful inertness in the interior—where, in fact I conceive that everything is motionless. Under these circumstances, and debarred from that obvious means of boiling our pot—the invasion of a circulating current, or casual injection of intensely hot liquid matter from below the question *"Whence comes the heat?* and *Why did it come"* remains to be answered on sound theoretical grounds. Now that answer I conceive to be as follows—granting an equilibrium of temperature and pressure within the globe, the Isothermal Strata near the Center will be spherical, but where they approach the surface will by degrees conform themselves to the configuration of the surface of the *solid* portion, ie the bottom of the sea & the surface of continents [fig. 1]. Suppose such a state of equilibrium & that under the bottom B of my great ocean DE the Isothermal strata are as represented by the black lines.[23]—Now let that Basin be filled with solid matter up to A. Immediately the equilibrium of [heat] **temperature** will be disturbed—Why? because the form of a stratum of temperature depends essentially on the form of the bounding surface of the solid above it—that form being one of the arbitrary functions which enter into its partial differential equa-

[21] Lyell gives some credence to these two hypotheses in *Principles* **2**: 370 and 365. Humphry Davy first proposed his alkaline oxidation theory in 1808, in Electrochemical researches on the decomposition of the earths, *Collected works* **5**: 139, London, 1840. However, his fullest statement is in On the phenomena of volcanoes, *Works* **6**: 355–357, a paper of 1828. He rejected the theory in his last book, written practically from his death bed, in favor of a hot nucleus of the earth: *Consolations in travel*, 138, London, 1830.

[22] Poulett Scrope, George, *Considerations on volcanoes*, 26–28, London, 1825.

* Heated liquids circulate not because the lower parts are *hotter* but because they are *lighter* than the upper—But in the interior of a heated globe the density depends not *only* on the *temper*ature but on the *pressure* (i e the depth) of each stratum so that nothing is easier than to imagine a law of increasing temperature which shall coexist with increasing density.—JFWH

[23] See figure 1.

tion. Immediately therefore, [heat] **the temperature** will begin to migrate from below upwards, and the Isothermal strata will gradually change their forms from the black to the dotted lines. The lowest portions at B then will (after the lapse of ages & when a fresh state of equilibrium is attained) have acquired their *then actual* depth—while a point as deep below B as C is below the surface, will have acquired a much higher temperature, and *may become actually melted, and that without any bodily transfer of matter in a liquid state from below.* But if C [was] be already at the melting point, B will now be so— i e the *lower level* will attain B, and the bottom of the new strata will melt, *water inclu* [Suppose now] *ded*, with which from the circumstances of the case they must be saturated.

10. Now let the process of Deposition go on until by accumulation of pressure on the bottom or sloping sides or on some protuberance from the bottom some support gives way—a piece of the solid crust breaks down & is plunged into the liquid below, & a crack takes place extending upwards. Into this the liquid will rise by simple hydrostatic pressure—But as it gains weight, it is less pressed, & if it attains such a height that the ignitid water can become steam, the case before alluded to arises—the joint specific gravity of the column is suddenly diminished—and up comes a jet of mixed steam & lava—till so much has escaped that the deposited matter takes a fresh bearing when the evacuation ceases & the crack becomes sealed up.—

11. In the analysis I have above given of the process of heating from below we have if I mistake not, a strictly theoretical account of that great desideratum of the Huttonian theory.[24] "Let Heat" says he "invade a newly deposited stratum from below!" But why??—not because great currents of melted matter are circulating in the nucleus of the globe—not because great waves of Caloric are rushing to & fro without a law & without a cause in the subterranean regions—but simply because the fact of new strata *having* been deposited alters the conditions of the equilibrium of temperature and they draw the heat to them, or which comes to the same thing, retain it *in* them in its transit outwards (the supply from the center being supposed inexhaustible & its temperature of course invariable.)

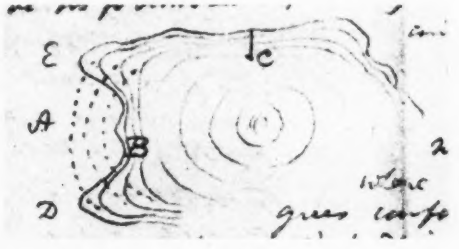

FIG. 1.

[24] Both Uniformitarians and Catastrophists were more or less followers of James Hutton in geology. Lyell was much more Huttonian than the Catastrophists, but Herschel in these letters is even more Huttonian than Lyell.

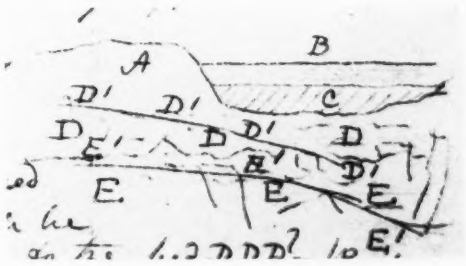

FIG. 2.

12. According to the general tenor of your book, we may conclude that the greatest transfer of material to the bottom of the ocean is produced at the coastline by the action of the sea, & that the quantity carried down by rivers, from the surface of continents is comparatively trifling. While therefore the greatest local accumulation of pressure is in the central area of deep seas—the *greatest local relief* takes place along the abraded coast lines—Here then in this view should occur the chief volcanic vents. If the view I have taken of the motionless state of the interior of the earth be correct, there appears no reason why any such influx of heat should take place under an existing continent (say Scandinavia) as to heat incumbent rocks [which retain their] (whose bases retain their level) 5 or 600° Fahr. for many miles in thickness (Geol. Vol 2,. p. 384) Laplace's** Idea of the elevation of surface due to columnar expansion (w^h you attribute in a note to Babbage) is, in this view inadequate to explain the rise of Scandinavia or of the Andes, &c. But in the variation of local pressure due to the transfer of matter by the sea, on the bed of an ocean imperfectly & unequally supported it seems to me an adequate cause may be found

13. Let A be Scandinavia [fig. 2]—B the adjacent ocean (the North Sea) C a vast deposit newly laid on the original bed D of the ocean E E E a semifluid or mixed mass on which D D D reposes.[25] What will be the effect of the enormous weight thus added to the bed D D D?—(Rock being heavier than sea)—Of course to depress D under it and to force it down into the yielding mass E a portion of which will be driven laterally under the continent A and upheave it Lay a weight on a surface of soft clay. You depress it below and raise it around the weight.—If the surface of the clay be dry & hard it will crack in the change of figure.

14. However you will be tired of me & my speculations & theories & I have yet much to say about your book—so much indeed that I have not now time to put on paper more than a very small part of what has struck me on reading it.—I hope your example will be followed in other sciences, of trying what *can* be done by existing causes, in place of giving way to the indolent weakness of a priori dogmatism—and as the basis of all further procedure enquiring what ex-

** Nisi Mitscherlich's—I remember well to have read it somewhere or other—JFWH.

[25] See figure 2.

isting causes really are doing. In one subject (I mean Philology) it strikes me this would be very desireable. An enquiry into the laws of verbal corruptions & into the process and rate by which words do actually change their meanings in the various existing states of society—with & without printing—with & without literature—as well as the changes of pronunciation would, I feel persuaded be a very productive line for any expert linguist to engage in & would tend to elucidate many difficult problems which the astonishing diversity of human speech presents. Words are to the Anthropologist what rolled pebbles are to the Geologist—Battered relics of past ages often containing within them indelible records capable of intelligible interpretation—and when we see what amount of change 2000 years has been able to produce in the languages of Greece & Italy or 1000 in those of Germany France & Spain we naturally begin to ask how long a period must have lapsed since the Chinese, the Hebrew, the Delaware & the Malesass[26] had a point in common with the German & Italian & each other.—Time! Time! Time!—we must not impugn the Scripture Chronology, but we *must* interpret it in accordance with *whatever* shall appear on fair enquiry to be the *truth* for there cannot be two truths. And really there is scope enough: for the lives of the Patriarchs may as reasonably be extended to 5000 or 50000 years apiece as the days of Creation to as many thousand millions of years. Nor indeed can I see much more wonder in a man living 10 or 20000 years than to the age of Methusaleh. Both the Egyptians of old & modern Chinese held a far higher notion of the antiquity of our race than we do—The Chinese records are assuredly not ephemeral—and the Pyramids were a matter of quite as much speculation in the days of Herodotus as at this moment. The confusion of tongues recorded in Scripture was a miraculous event, and unless we draw some line & agree to limit the miracles in their extent & effects to the parties immediately concerned in them (where the contrary is not expressly said) I do not see how we are to avoid admitting that the earth revolved backwards on its axis 15° in the time of Hezekiah & more in that of Joshua, as physical facts, and essential features in Astronomy, **which ought to be taken account of in the calculation of the Chaldean eclipses**

15. In p. 35. Vol. 3. you speak of the germination of seeds after undergoing great heat or even actually being boiled.[27]—I can give you some facts. I have here sown the seeds of the Acacia Lophanta after stewing them for 12 hours [in] over a lamp in water of 140° Fahr. (never below—often above—& sometimes quite scalding) They germinated far more rapidly than unboiled seeds.—The seeds of the Protea Argentia will lie several years in the ground without germinating—but if the seeds be sown ½ inch deep and then the ground be burned (which here happens yearly & makes a fierce fire of the dry bulb-leaves &c) they come up at once. I last year sowed nealy an acre with them, first parboiling them. In one instance by inadvertence I absented myself about ½ hour & on my return found that the water had boiled violently in an outer vessel, within which the seeds were placed in a tin cup, **with water.** & had no doubt attained the boiling temperature. They germinated vigorously, & are now nearly a foot high. Baron Ludwig an eminent Botanist here (who is going soon to England) will give you other cases—especially of a species of cedar whose seeds he could not get to grow till *thoroughly Boiled*.

16. In page 115 Vol 3 line 14 there is an inaccuracy of wording easily rectified by inserting between the words "all" & "fertile"—the words "female, and all those females" [fertile"] and in the next line between the words "produce" and "more"—the words "in the next generation."—[28]

17. Changes of Climate.—Dr. Andrew Smith, just returned from the tropic of Capricorn—assures me that the River at Kuruman (Latakoo) has now almost ceased to run. 50 years ago it was a great & rapid River up which abundance of Hippopotami came.—His informant on the spot a native pointed out a spot where he had shot many Hippopotami, where now the bed of the river is always dry or at least no more water runs than you can pour by hand with a jug from one puddle to another.—Great & copious springs which never used to dry summer or winter are quite extinct & no water is to be found on digging 20 feet.

18. A similar case occurs at Grigua Town. There it has ceased to rain altogether—the seasons having grown progressively more & more dry. In consequence *their* river is now nearly dry & the desertion of the place appears certain.

19. I am sorry that my astronomical pursuits have not yet suffered me to view the hot springs of [Caledon] **the Brayed Valley.** It is described as a copious river of boiling hot water equally full at all seasons issuing from a single source—and perhaps the greatest hot source known.

20. Last month I made a second visit to the Summit of the Paarl Rock a huge rounded, weathered mass of granite of beautiful uniformity of texture & enormous dimension It is intersected from side to side by a granite dyke which so far as I know is unique, being subdivided by oblique cross cleavages just as lava dikes often are—which give it, where it projects from the surface in consequce of its superior hardness—

[26] There was no fixed usage at this time concerning the name of the language of the inhabitants of Madagascar. James Prichard, in his classic *Researches into the physical history of mankind*, 3rd ed., **5**: 191–195, London, 1836–1847, gives as possibilities Malagasy, Malgaches, Madecassas, Malacassas, and his own favorite Malecasses.

[27] Lyell included the information in this paragraph in his next edition; *cf. Principles*, 5th ed., **2**: 17, Philadelphia, 1837.

[28] Lyell had written: "Reamur observes that the female moth lays about four hundred eggs, so that if twenty caterpillars were distributed in a garden, and all lived through the winter and became moths in the succeeding May, the eggs laid by these, if all fertile, would produce more than three million moths." He revised the passage along the lines of Herschel's suggestion; *cf. Principles* 5th ed., **2**: 63–64. This correction is another bit of evidence that Herschel had been reading the book quite closely.

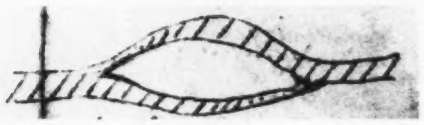

FIG. 3.

exactly the appearance of a wall built of oblique bricks [of a] It bifurcates in one part of its extent & closes again, but in a curious way the offset branch begins very small & enlarges to the full original dimension while the principal trunk diminishes by the same degrees & trains off to a very small thread when it reunites to the offset [there].[29] The greatest dimension [fig. 3] is about 12 or 14 inches across It is of the same granite as the general mass, but infinitely smaller grained & much harder.—

21. Speaking of Dykes—At Simon's Bay there is a beautiful dyke of Black Basalt cutting for a great distance through the granite wh forms the base of all the Table Range as far as Cape Point—It is laid bare on the sea shore (1 mile ± south of Simons Town) for about 40 or 50 yeards—breadth 20 inches, makes one or two very sudden flexures—appears to be vertical—Is black basalt very hard & magnetic—exhibits a tendency to columnar cross structure, or rather is laminated across the dyke, the laminations larger in middle smaller at the sides thus [fig. 4].[30]—B. In **the** [stone] granite cut through by this dyke are *imbedded masses* **of stone** much like hornstone in aspect, hard & tough *and evidently angular fragments* somewhat rounded as if by partial fusion.—They occur so firmly fixed, & so completely part of the stone worn down to the general surface that it is impracticable to detach them, besides which they are of considerable size. I do not know if this be new in granite—I never recollect to have seen it. In p. 197 of Vol 2. you give an account of a formation of Travertine going on the coast of Lancerote which you attribute, no doubt justly, to the sea spray. I can give you a case in point—At Simon's Bay (and also as I am told at many other points of the coast) there occur great banks of sand (mixed with abundance of shells) the surfaces of which are as it were bristled over with what seem to be petrified stumps of trees which especially affect the form of a very common shrub here the "Krüppelbaum" or Leucospermium. . . . These when examined may be traced down to considerable depths and branch out like roots. They are composed of sand cemented by calcareous matter & run from the thickness of a straw to that

FIG. 4.

[29] See figure 3.
[30] See figure 4.

FIG. 5.

of a mans arm & more. They are usually (at least the large ones) hollow or loosely stuffed with woody matter in a state approaching to Charcoal. It is rare to find within them undecayed wood but I have one unequivocal specimen in that state—not however a single stick—but an aggregate of small ones—evidently a bundle of roots. The large sticks are pretty compact—but the small ones less so. They are often found interlaced into a fibrous mass of very delicate texture like very small branches interwoven. In this state it is hardly possible to transport them in safety. I mean to send you specimens when I can find an opportunity without putting you to more expense than the thing is worth, or at all events shall bring some home with me.—You seem to think a volcanic neighbourhood essential to this sort of formation & it is odd enough that this is the locality of the only unequivocal basaltic dyke which I have seen—for what is here called by that name, in the Klass between the Table & [Devil] **Lion** mountains so far as I have examined it looks very suspicious being a loose mass of friable ochrey matter which seems to have filled in a crack from above. Monsr. D'Abadie of Mauritius [writes me] informs me that a formation of calcareous matter which from the description must be much of the same nature as that at Simon's Bay, is going on in some of the little islands adjoining Mauritius—. I have myself found recent shells firmly cemented in the calcareous deposit which fills the chinks between the Basaltic Columns of the Grand Farashire of the Cyclopean Islands, & have somewhere at home the specimens I detached from that locality.

22. Vol. 4. page 97. Contortion of strata.[31]

Let C [fig. 5] be the slope of a hill either projecting above an ocean A or wholly submerged beneath it and let soft strata be deposited all over the bottom. For a time these will settle equally on the slope & bottom but as the weight increases they will at length slide down in a soft or semihard state and become crumpled up [fig. 6] especially if in their slope they abut on some obstacle which forces them to yield and double up as at D [fig. 7].

23. Speaking of taluses of mud deposited on slopes of submarine hills—I should mention that by far the

FIG. 6.

[31] See figures 5, 6, and 7.

FIG. 7.

most striking feature of the geology of the Cape is the enormous deposits of ferruginous clay (evidently resulting from the mingled detritus of the granite [& sandstone] slate, & sandstone of which all the mountains here consist) which forms a think bank all around the lower slopes of the mountains both on this & the opposite side of the great valley of the "Cape Flats."[32] It is furrowed out by the rains into channels of vast depth which disclose a perfect uniformity of composition [fig. 8]. I have never found the most trifling fossil in it. Indeed in that respect nothing can be more monotonous than this district, both the slate and super-incumbent, unconformable Iridacean sandstone being entirely destitute of organic remains. It is however clear that the sea once stood at a much higher level here than at present as the anexed sketch will shew.[33] A being the Table Mount" & B [that] those[34] of the opposite **side of the valley**.—exhibiting steep cliffs at A, B, and a talus at a, b [fig. 9].—C is the Klapmuts cone whose rugged summit C perched on the smooth conical talus c, attains *just the height* where the mural cliffs commence on both sides of the valley.—d is the "Tyirberg"—*just wholly below* that range of level & whose curvature though somewhat steep at top, is [never] **nowhere** broken by any *precipise*. e is the "Saxenberg hills" much lower than the Tyirberg—& in the same proportion more smoothly rounded & less abrupt—while e[35] is Robben Island a perfectly waterwashed bank a mere shoal.—Ff is the Lions head & Rump of which the precipitous part corresponds exactly in height to that of its next neighbour the Table Mountain. Nothing can I think prove more clearly a former sea-level at x *gradually* subsiding to the present level gg

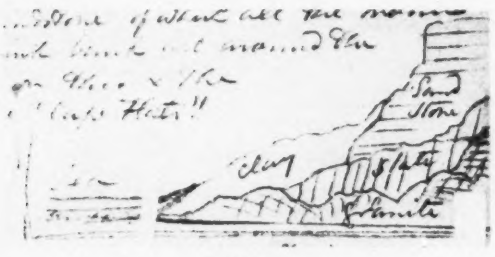

FIG. 8.

[32] See figure 8.

[33] See figure 9.

[34] For this and for one other difficult reading (note 45) I wish to thank Dr. F. J. Pequigney of the English Department of the State University of New York.

[35] Herschel should here have written "f", referring to the "f" near the bottom of the sketch.

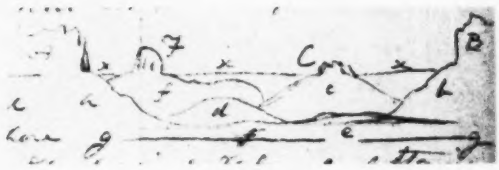

FIG. 9.

—but a very few feet below the existing level of the great flat plain of sand which extends from [Simons to] Table to False Bay.

24. P. 361. Cleavages of Rocks.[36]—If Rocks have been heated to a point admitting a commencement of crystallization, ie to the point where particles can begin to move inter se—or at least on their own axes—some general cause must determine the position these particles will rest in on cooling—probably that position will have some relation to the direction in which the heat escapes.—Now when all—or a majority of particles of the same nature have a general tendency to one position that must of course determine a cleavage plane.—Did you never notice how the infinitesimal crystals of fresh precipitated sulphate of Baryta & some other such bodies—arrange themselves alike in the fluid in which they float so as, when stirred all to glance with one light & give the appearance of *silky filaments*. Ask Faraday to shew you this phenomenon if you have not seen it—it is very pretty. What occurs in our expt. on a minute scale may occur in nature on a great one, as in granites, gneisses, mica slates &c—some sorts of soap in which insoluble margarates exist shew it beautifully **when mixed with water.**

25. Well. I really *must* hold my hand—at least for the present—but your book has stirred up my brains—& every page I turn up brings up some fresh *ooze* from their dark deposits—I don't know whether I have made clear to you my notions about the effects of the removal of matter from [contin] above to below the sea.—1st it produces mechanical subversion of the *equilibrium of pressure*.—2dly it also, & by a different process (as above explained at large) produces a subversion of the equilibrium of temperature. The last is the most important. It *must be an excessively slow process*, & it will depend 1st on the depth of matter deposited—2d on the quantity of water retained by it under the great squeeze it has got—3dly on the tenacity of the incumbent mass—whether the influx of caloric from below—which MUST TAKE PLACE acting on that water, shall either heave up the whole mass, as *a new continent*—or shall crack *it* & escape as a submarine volcano—or shall be suppressed until the mere weight of the continually accumulating mass breaks its lateral supports at or near the coast lines & opens there a chain of volcanoes.

26. Thus the circuit is kept up—The primum mobile is the degrading power of the sea & rains (both originating in the suns action) above and the inexhaustible supply of heat from the enormous reservoir

[36] Lyell incorporated this paragraph almost verbatim in his next edition: *cf. Principles* 5th ed. **2**: 486–487.

below always escaping at the surface unless when repressed by an addition of fresh clothing at any particular part.—In this view of the subject the *tendency is outwards*. Every continent deposited has a propensity to rise again & the destructive principle is continually counterbalanced by a reorganizing principle from beneath.—Nay—it may go farther there *may* be such a tendency in the globe to swell into froth at its surface, as may maintain its dimensions in spite of its expense of heat & thus preserve the uniformity of its rotation on its axis in spite of M. Cordier & his refrigerating & contracting doctrines (which by the bye had occurred to myself & been rejected as inadequate [as] **to give** a general formula of explanation of volcanos &c. long before I ever heard the name of Cordier.[37] Perhaps I shall recur to this subject on some future occasion—but really the stars leave me very little time to lick into form any geological theories or even to examine them with any **degree of** scrupulous severity.

27. I hope you are well and do not work yourself too hard. I am getting on well with my astronomical work & hope to finish it in another year or thereabouts—we propose to return via Brazil. **Can you give me any hint, information or introduction, at Rio?** In some respects I shall be sorry to quit the Cape. It is a delicious climate & agrees admirably with the children. But still it is an insidious one for adult Europeans—not to be sure so bad as India—nor does it affect general health—but it claims a tax of bodily suffering in form of Rheumatic complaints that is tremendously severe. Nor can this be wondered at when the sudden & excessive changes of temperature are considered. It has occurred to me to see the thermometer at midnight at 41° Fah and to be complaining of cold—and by 2h AM on the **same** morning to have it standing on my writing desk in the free air 15 or 18 feet above ground, at 71° Fah. A fall of 20° in the evening within an hour or two after sunset is a matter of no rare occurrence. Against such changes no precaution of dress can avail.

28. Remember me to all my friends at the Geological [38]—Sedgwick, Murchison—Fitton—Buckland—Stokes—Broderip & all of you—Tell Murchison I have some Trilobites for him from the Cedarberg 120 miles from Cape Town. N. E. which though not very fine specimens are the best I am likely to procure—being obliged to trust to casualties for obtaining them as I have not visited, nor am likely to visit the spot whence they come.—I sent Buckland a few Luminous Medusae which I fished up coming out—creatures as big as my thumb, bag-shaped, and intensely luminous when fresh caught and squeezed. Ask him if he has rec[d] them.

29. I beg my best respects of M[rs]. Lyell though personally unknown to her—and am my dear sir Yours very truly

JFWHerschel

[37] *Cf.* Cordier, L., Essai sur la temperature de l'intérieur de la terre, *Mémoires de l'Académie Royale des Sciences* **7**: 473–555, 1824; abstracted in *Edin. New Philos. Jour.* **4**: 273–290, 1827–1828.

[38] The Geological Society of London.

III. THE 1837 LETTER

Aside from its general importance as suggested in the earlier parts of this article, Herschel's letter caused a bit of a stir among his friends in London because of his casual dismissal of Babbage's priority in paragraph 12: "Laplace's idea of the elevation of surface due to columnar expansion (w[h] you attribute in a note to Babbage. . . ." Lyell wrote back a long letter thanking Herschel for his praise, stating that Herschel's approval of the climate theory in the *Principles* was worth more than the fact that the fourth edition had sold out in a year, and asserting,

> I may truly say that when the Royal Society voted me a medal for my book, I was not more gratified nor more encouraged than by your full and interesting comments which have given me a feeling of strength and confidence in myself, which will assist me in my future studies.

He went on to say, very tactfully,

> Your very interesting volcanic theory was too much for me to attempt to grapple with, as an intercalation into my book, at least this time; but I mean to have a work at it this summer as soon as I have got through the reprint. It struck me on first reading as singularly like a speculation of Babbage's, which he appended to a paper of his on the Temple of Serapis; and of which an abstract appeared in the 'Proceedings' of the Geological Society which I suppose you have, although in case you should not, I will send you a copy if I can procure it.[39]

Roderick Murchison also wrote Herschel, but both letters produced only a letter from Herschel to Murchison explaining how his views were quite different from "some theory ascribed to Mr. Babbage (but, I believe, before propounded by Mitscherlich) about the elevation of strata by *pyrometric expansion* of the subjacent columns of rock." [40]

Finally Lyell took the bull by the horns and wrote to Herschel quite plainly, on May 24, 1837:

> You seemed to think that I had confounded this [Herschel's] notion with Mitscherlich's expansion of stone by heat, attributed by me to Babbage; but in fact Baggage's theory, which I had alluded to, was different, and in substance the same as yours, and I agreed with Murchison, that as Babbage was going to republish it in his 'Ninth Bridgewater,' it would be well to allow him to print, as he desired, both the extract from your first letter to me on the point, and your note to Murchison especially, as in both you had

[39] Lyell, Charles, *Life letters and journals* **1**: 464–466, London, 1881.

[40] Babbage, *Ninth Bridgewater Treatise*, 214; the letter is abstracted in *Proc. Geol. Soc. London* **2**: 550–551, 1833–1838.

stated that you had not had time to reason out or state with minute accuracy the whole question. Whewell had both letters read at the Geological Society, which produced a most animated discussion in which Whewell took part.[41]

Then Lyell went on to explain that he had permitted publication of Herschel's opinion as to the naturalistic origin of species:

> Babbage was very desirous that I would allow him to print another short extract from your letter to me. I objected at first, but he showed me that I need not be alarmed, because he introduced it as a counterpart of a passage from Bishop Butler, and that in such company no one could be otherwise than correct and orthodox.
>
> I hope my willingness to be persuaded to have a passage printed, in which incidentally you had paid me a compliment (one which I certainly prized highly), has not led me to what you would in any way think an indiscretion.[42]

This letter produced an immediate reaction in Herschel which he describes in the letter reprinted below, of June 12, 1837. Paragraphs 1, 3, 4, 5, 6 and 7 were read to the Geological Society on January 31, 1838, and appear in abstract as Herschel, On internal temperature, *Proc. Geol. Soc. London* **2**, 596–598, 1833–1838. These paragraphs were printed in the second edition of Babbage's *Ninth Bridgewater Treatise* in 1838, pp. 241–247, along with the excerpts from the two earlier letters. The first and the last five paragraphs, however, have never been printed.

The manuscript of this letter in the Library of the American Philosophical Society consists of one four-page sheet similar to those of the earlier letter (but on tan rather than gray paper), and one two-page sheet with writing on one side only. Inspection of this half-sized sheet reveals that it has been torn along the crease, and hence that two full pages of the letter are missing. This is confirmed by the content, which breaks off at the end of the fourth page with "(see" and then takes up on the surviving half-sheet with a totally different subject. It is impossible even to guess as to why the two pages are missing; the only evidence is that the separation was done quite carefully, and hence deliberately. The final page contains enough material to make one greatly regret the missing pages. There is the slightly belittling remark about Henry de la Beche's book, together with the deliberately ironic running together of its title; the discovery that a young *geologist* like de la Beche would send a copy of his book to John Herschel the astronomer; the plea of not enough time, immediately followed by the results of a new geological field trip; and the graphic description of forest fires at the Cape with only a casual reference to a "narrow escape" for Herschel's own house. Above all, there is Herschel's announcement of the rediscovery of the sixth satellite of Saturn. The original discovery of this satellite has been that one of William Herschel's observations which had been received with the most skepticism, since no one else was able to observe it. That John Herschel could say, "This has given me more satisfaction than anything I have done at the Cape," shows the depth of the filial sense of responsibility which, one might guess, more than anything else kept John Herschel from doing anything as original in his own way as his father had done in his.

Equally important, John Herschel's rediscovery of the satellite shows one important reason why the Herschels dominated sidereal astronomy for two generations. They simply had the best equipment. Not until Lord Rosse built his giant telescope in the 1840's, and with it discovered the spiral nature of many nebulae, could anyone else see in the heavens what William Herschel and what John Herschel could see.

IV. REMAINING PORTION OF THE 1837 LETTER

Feldhausen June 12. 1837

My dear Sir,

1. Your note introducing Lieut H. J. Thomas R A. has been forwarded to me by him, and I shall in consequence be very happy to make his acquaintance & shew him any attention which the now, necessarily very limited intercourse we hold with general society in consequence of the daily increasing burden of my

[41] The letters were read at the meeting of May 17, 1837. William Whewell had been recruited by Lyell to follow Lyell as president of the Geological Society. Uniformitarians and Catastrophists were united in their desire to keep control of the Society out of the hands of the older geologists, although they failed to find an adequate successor to Whewell and had to acquiesce in the election of William Buckland. There is an unpublished manuscript having to do with this politicking in the Darwin-Lyell collection of the American Philosophical Society, a letter of Roderick Murchison to Lyell of September 22, 1836. Murchison approves of Lyell's asking Lord Northampton to be president and assures Lyell that he is not committed to Buckland. Indeed, he is quite disgusted with Buckland's recent buffoonery at Bristol (the meeting of the British Association for the Advancement of Science), and during the meeting Adam Sedgwick became so annoyed at Buckland that he "said that he was determined to speak at length that night 'to take science out of the *dirt* into which B had shoved it'!!!"

[42] Lyell, *Life* **2**: 11–12. The reference to Bishop Butler in Babbage's *Ninth Bridgewater Treatise* is on p. 175, in a note to chapter VIII on miracles.

astronomical reductions will admit. In fact I have now made it a principle to go *now*here except on some distinct point of business & do not visit Cape Town once in a month or two.

2. I reply to your note however immediately because there are points in it & and in a letter of Murchisons I lately rec[d] but especially in y[rs] which call for immediate notice on my part lest I should be supposed to have willingly & knowingly what our French neighbours call "emprunté des idees" appropriated, the [theories] **ideas** of Babbage to which charge should any one feel disposed to bring it (B. I am sure will not)—I plead not guilty. Till the arrival of Murchison's letter in fact I was utterly unaware that Babbage had or (anybody else) speculated on that peculiar **mutual** reaction of the surface and interior of the globe which consists in which I think we must now call "the secular variation of the Isothermal surfaces" [in the interior of our globe] of the latter. The idea I considered as entirely my own and I was never more taken by surprise than when today, directed, for the first time by an express mention in your letter of a paper of Babbages abstracted in the Geological notices, I hunted up all those notices in my possession and found, in an uncut N° (N° 36) (as I am sorry to say many of these & other not less interesting brochures which have reached me, still are)—an abstract of his paper on the temple of Serapis at the end of which a theory identical with mine in that leading point undoubtedly stands printed.

3. Convinced as I feel of the great [geological] importance of this general view of geological revolutions in contrast with all the arbitrary [and] local & temporary expedients which have hitherto been resorted to to explain particular phaenomena, and to the recourse had to "the volcano & the earthquake" as the great ex*plaining* powers—whereas in this view these are only symptomatic phaenomena, natural and necessary concomitants of systems of action much more extensive, w[ch] are constantly going forward.—It was not I confess without some slight disturbance of equilibrium, & a momentary coincidence of feeling with that of the philosopher who said "pereant qui ante nos nostra &c" that I read this—but God forbid that I should for an instant indulge any shadow of wish to rob Babbage of one tittle of what belongs to him. But as I do, at all events, lay claim to absolute independence of speculation, on the subject, it is quite right that I should make clear to you and to him the progress of my own ideas, and also account for what must have appeared singular in my own mention of his speculations in my letter to Murchison. And to take the last first—the fact is that I never was aware that Babbage had made *any* communic[n] to the Geol. Soc. on the subject till Murchison's letter first led me to suppose and yours expressly stated, by referring to the Proceedings for an abstract—that such was the case—The [note] Passage in your book* & note appended (Vol 2 p. 383) contain no allusion to the *cause* of rocks becoming heated from below—The employment of the **pyrometric** expansion of rocks as a motive power was, I feel confident suggested by *some* one (& the name of Mitscherlich, *or* Laplace, has somehow got connected in my memory with it) many years ago—certainly before 1833: Of this B. must have been as ignorant as I was of his views, as he appears to have based his ideas on Col Tottens experiments (when made I know not). And [as] I only remembered to have read Babbages paper on the temple of Serapis published in one of the quarterly journals not long after his arrival from the Continent in w[ch] so far as I recollect this point is not touched upon, nor is it, in your speech from the chair,[43] where alternate pyrometric expansion and contraction without reference to the cause of the invasion or abstraction of heat, are alone alluded to.

4. However discussion of points of this sort is of little moment in itself, as if a theory be founded in sound views it matters little to the world whether A or B or A *and* B first entertained it—or whether it arose in the whole Alphabet when its seeds were ready to germinate. As regards the course of my own ideas [they were] it was simply this. When I first read your book I was struck with your views of the Metamorphic Rocks, and I began to speculate how & why the mere fact of deep burial might tend to raise their temperature to the required point—Three modes occurred 1[st] developement of heat by condensation—but this cause seemed somewhat feeble and not very clear in its mode of action, since at every moment an equilibrium of pressure & resistance is established—2[dly] plunging down into an ignited pasty mass.—Here however, considering the excessive slowness of the process it occurred to me that there would be plenty of time for the ignited matter below, not merely to divide its caloric with the newly superposed mass, but to take up fresh from below & thus to establish a regular gradation of temperature from below upwards—And this led to the 3[d] & more general view of the matter which is that of the variation of Isothermal surfaces, as stated in my letters to Murchison & yourself.

5. These notions had been fermenting and regurgitating in the cavities of my brain from the moment I first read your metamorphic doctrines in your first Edition—but what determined the disruption of the incumbent stratum & their final explosion was the reperusal of your little 18[mo] Edition you were so good as to send me.

6. All things considered however, I do not regret having written what I did, and I am still so far disposed to regard it as publici juris as to wish that such passages in my letters as yourself and Murchison may think eligible for the purpose, might on some fitting opportunity be read at a meeting of the G. S. (all idea of my drawing up a regular paper is out of the question, I am so[o] involved in other matters at present). It will draw attention to the subject and science will gain by the discussion. When people think independently, at different times & excited by different original subjects of considerat[n] bearing on one more general object—If their ideas converge towards one view of the matter, it is a proof that there

* [Neither does your speech contain any allusion to this point of his theory.]—JFWH

[43] Lyell, Charles, Presidential address of 1836, *Proc. Geol. Soc. London* **2**: 380, 1833–1838.

is something worthy of further enquiry—& if they think to any purpose it is hardly possible but that many points will occur to Each which do not to the other, & that so a theory may branch out and acquire a body much sooner than it would do by the speculations of one alone, and indeed such is in some degree the case in the present instance. B. for example has speculated not only on the heaping on of matter in some parts, but on its abstraction in others as a cause of variations in the Isothermal surfaces—and justly —It is a case of the algebraic passage from + to − passing through 0. In envisaging (as the French call it) the question algebraically the cases could not be separated. Again he has confined himself to the [exp] pyrometric changes in the solid strata while I have left these out of view and relied on what I think to be a far more energetic & widely acting cause—the variation of pressure and the infirmity of supports broken by weight or softened by heat, to produce *tilts*. Both causes however doubtless act & both must be considered in farther detail—the former **alone** may account for the phaenomena of the Bay of Naples—the latter **must I think be called in to account** for those of Scandinavia **and Greenland** and of the Andes.

7. I would observe that a *Central* heat may or may not exist for our purposes—but it seems to be a demonstrated fact that *temperature does* in all parts of the Earths surface **yet examined** increase in going down *towards* the Center, in what I almost feel disposed to call a *frightfully* rapid progression—and though that rapidity may cease, & the progression even take a contrary direction long before we reach the Center (as it might do for instance had the Earth, originally cold, been as Poisson supposes, kept for a few [m]Billions or Trillions of years in a firmament full of burning Suns besetting every outlet of heat and then launched into our cooler milky way)[44]—still as all we want is no more than a heat sufficient to melt silex &c I do not think we need trouble ourselves with any enquiries of the sort, but take it for granted that a very moderate plunge downwards in proportion to the Earths radius will do all we want. Nay the Internal heat may even be locally [(i e confined to some still] **unequal** i e great in Europe & Asia—small under America as it would for example if when Roasting at Poissons Sun-fire, the great Jack of the universe had stood still and allowed one side of our Terraqueous joint to scorch & the other to remain underdone—a hint to those who are on the look out for a cause (if any such there be) for the "Poles of maximum cold" and the general inferior temperature of the American climate from end to end of that continent (See . . .

. . . . .

. . . . .

[44] Poisson, Simon, *Théorie mathématique de la chaleur*, 3-4, 438, Paris, 1835.

8. I have to ask of you as a favor if you should see or communicate with Mr delaBeche, to thank him for his work "HowtoobserveGeology" (what an odd title) which he was so obliging as to [communicate] send me. It reached me at a time when I was in the thick of a chaos of obs^{ns} &c in a series of Glorious weather & I could not then make time to write to him in thanks, & have been angry at myself ever since whenever I have set eyes on the book. It is equally impossible for me now to write. Indeed my friends now must not quarrel with me for apparent negligence in that respect as every moment forces me to *throw overboard* something—or I shall *never* get home.

9. In my last to you, I mentioned certain Dykes in the Granite between Table M^{n}. & Lion's Head which Abel mentions but which I thought problematic. I have since examined farther & find his account correct —what I saw was only one & that the least marked of the whole, & would certainly authorise a certain scepticism.—There are phaenomena in the Granite there w^{ch} I have not time or room to describe which have I think escaped his notice & are very curious.

10. I am very sorry to see much of your letter not in your own hand—ie for what I know is the *cause*. I trust however the weakness of your eyes does not go on increasing & that you remain otherwise generally in good health.

11. I have not, of course much of interest to communicate as to what passes here—Stars stars stars and Fires, fires! fires! Such conflagrations of forests! Conceive the whole range of the Table Mount. from end to end one bright blazing with long festoons and Cordons of flame about midheight in the precipices—5 or 6 miles at once while the summit was wrapped in cloud, bloody-red with the glare, and disclosing occasional flaming points high upon the summit & mingling with the stars. Such was one of 3 or 4 grand igneous spectacles with which we have been treated—in one of which our own house had a most narrow escape.

12. My latest Astronomical news is the Rediscovery of the 6th satellite of Saturn (the 2^{d} in order of the 7 from the Planet) Since my Fathers observation I believe no one has been able to procure any decisive evidence of its existence further than by his report.[45] I have now several good observations of it & have traced it round & round, many revolutions. This has given me more satisfaction than anything I have done at the Cape.

Believe me, my dear Sir
Very truly yours JFWHerschel

[45] This word was accurately reconstructed by Mr. F. J. Pequigney (*cf.* note 34) in spite of a hole in the manuscript.

THE GOVERNOR AND THE WESTERN RECLUSE: DE WITT CLINTON AND FRANCIS ADRIAN VAN DER KEMP

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In September, 1820, on his sixth trip into central New York on business for the Canal, De Witt Clinton took time out to go hunting in the neighborhood of Olden Barneveld (now Trenton). Two old and surprisingly cultured gentlemen, angling for trout, hailed him in the friendly manner of Izaak Walton and invited him to share their catch. Handing over his bag of woodchuck, snipe, and ducks, Clinton went home with them to a dinner which he later compared to Plato's Symposium.

Both these fishermen were Dutch *émigrés*, banished from the Netherlands in 1787 when the Dutch patriots were defeated by the Orange Party, with the help of the Prussian army. Adam Gerard Mappa, a colonel in the Dutch patriot army, became prosperous as agent of the Holland Land Company in New York State. Francis Adrian Van der Kemp, clergyman, served only briefly in the army, but his pen and voice were feared by the Orange Party.

Using a romancer's privilege, Clinton wrote of the country banquet in his Hibernicus *Letters* as if this were his first meeting with Van der Kemp.[1] Certainly they had met at least once before,[2] but Clinton's romantic relocation of fact may be justified on the assumption that in this meeting he really came to appreciate Van der Kemp's genius. Clinton describes the impact of this encounter upon his mind and heart:

> Thus, my friend, I have made a great discovery. In a secluded, unassuming village, I have discovered *the most learned man in America*, cultivating, like our first parent, his beautiful and spacious garden with his own hands—cultivating literature and sciences—cultivating the virtues which adorn the fireside and the altar—cultivating the esteem of the wise and the good—and blessing with the radiations of his illumined and highly gifted mind, all who enjoy his conversation, and are honoured by his correspondence.[3]

The mutually productive friendship, sustained by a few more conversations on Clinton's western visits, but chiefly nourished by correspondence, lasted until Clinton died.[4] Neither would have called the other his closest friend; for this honor, Clinton would probably have chosen David Hosack or Thomas Addis Emmet; Van der Kemp, John Adams or Adam Gerard Mappa. But Van der Kemp supplied the erudition that Clinton admired but had no time to achieve, while Clinton represented the active, strenuous life from which Van der Kemp had long since withdrawn. Not only do their letters provide warm personal comments on happy and sad family news and reports of mutual friends;[5] but, what is more significant for the historian of ideas, they present a cross section of the intellectual and practical interests

[1] Hibernicus [De Witt Clinton], *Letters on the natural history and internal resources of the state of New York*, 216–219, New York, 1822.

[2] Van der Kemp's biographer suggests that they may have met at the house of De Witt's uncle, George Clinton (Fairchild, Helen Lincklaen, *Francis Adrian van der Kemp, 1752–1829. An autobiography, together with extracts from his Correspondence, edited, with an historical sketch by Helen Lincklaen Fairchild*, 182, New York, 1903). Van der Kemp began correspondence with Clinton Sept. 14, 1812.

[3] Hibernicus, *Letters*, 219.

[4] Except where otherwise noted, letters quoted or cited are from the De Witt Clinton Papers, Special Collections, Columbia University Library. Grateful acknowledgment is made to Roland Baughman, Head of Special Collections, and the Columbia University Library, for the use of this material.

The last items in this correspondence are V. d. K. to C., Jan. 29, 1828, and C. to V. d. K., Feb. 11, 1828, the day of Clinton's death. Because of Clinton's care in preserving his papers, we may assume that the Van der Kemp side of the correspondence is fairly complete; gaps appear in the Clinton side chiefly for letters written while on the road.

[5] For a time Clinton performed the services of banker and commission agent for Van der Kemp, buying supplies and lottery tickets from a sum of money entrusted to him by his friend (V. d. K. to C., March 4, 1824, and C. to V. d. K., April 2, 1825). Clinton gave Van der Kemp a portrait of himself which greatly impressed Mrs. Josiah Quincy when she called at the Van der Kemp home. "Oh! that is Clinton!" she exclaimed (V. d. K. to C., July 28, 1824, quoted in Fairchild, *Francis Adrian Van der Kemp . . .*, 186). When Fairchild edited Van der Kemp's autobiography (1903) this portrait was at the Oneida Historical Society in Utica. It has since disappeared from view, unidentified.

which absorbed New Yorkers during the first quarter of the nineteenth century.

Clinton was fifty-one in September, 1820; Van der Kemp, sixty-eight. At first sight, their differences in personality and background so strike the eye that the likenesses are obscured. Recently reelected governor, still the subject of bitter attacks chiefly instigated by Martin Van Buren, Clinton stood in the center of New York's political arena; while Van der Kemp, though possessed of some political experience and sagacity,[6] was now content to view politics through the long perspective of history. Although Clinton was indeed a favorite son of the State, having had the substantial advantage of support from his uncle George and the Livingston family at the outset of his career, he was nevertheless obliged to work up through the ranks: from United States Senator (then a less influential position than the state offices), to New York Assemblyman and Senator, to Mayor of New York, to Lieutenant Governor, to Governor. Clinton might well have envied the *émigré* Van der Kemp, who arrived in New York in 1788, armed with letters of introduction from John Adams and Thomas Jefferson, and proceeded to visit Governor William Livingston at Elizabethtown, Benjamin Franklin in Philadelphia; Adriaan Valck, consul of the United Provinces in Baltimore; and George Washington at Mount Vernon.[7] Van der Kemp's education was far superior to Clinton's. Clinton's activity as a public benefactor and statesman brought him correspondence with such keen minds as Thomas Jefferson, Benjamin Silliman, Jeremy Bentham and José Correa da Serra; but a glance at the faculty of the newly organized Columbia College, whose first entrant Clinton became in 1784—Benjamin Moore, John Gross, Samuel Bard, Henry Mapes, John Kemp—seems pitiful when compared with Van der Kemp's opportunities at Groningen University, under such scholars as de Rhoer, Schroeder, Le Sage, Ten Broeck, Widder; Camper, with whom he studied botany; Chevalier, ecclesiastical history; Van der Marck, ecclesiastical laws; and at the Baptist Seminary in Amsterdam, Osterbaen, theology; Wittenbach, Greek.[8] Both men held the honorary degree of doctor of laws, although Van der Kemp's, granted by Harvard in 1820,[9] conferred more distinction than that of Clinton, given him by Queen's College (now Rutgers), in 1812.[10] Nor could Clinton, however deep and earnest his Presbyterian piety,[11] claim the theological knowledge or the original speculation of the Hebrew scholar Van der Kemp. While still a student at Groningen, Van der Kemp was tried for heretical opinions, and he had worked through to a Unitarian position while minister of the Mennonite congregation at Leyden (1777–1780).[12] Yet even in the field of theology, where

[6] John Adams wrote of him to Samuel B. Malcolm, Quincy, Aug. 6, 1812: "Judge Van der Kemp is a great man, a star of the first magnitude under a deep cloud" (*The works of John Adams . . .*, ed. Charles Francis Adams, 10 v., **10**: 22, Boston, 1850–1856). (Adams used the term "judge" because Van der Kemp was for a brief period justice of the peace in Oneida County.) Sending a biographical sketch of Van der Kemp to Thomas Jefferson (Quincy, Aug. 9, 1816), Adams said: "You may well suppose that such a man has not always been able to understand our American politics. Nor have I. Had he been as great a master of our language as he was of his own, he would at this day have been one of the most conspicuous characters in the U.S." (*Ibid.*, **10**: 224–226).

Van der Kemp to Morris S. Miller, 14 Dec., 1814 (New York State Library) shows that Van der Kemp, irritated by the poor organization of the army and annoyed by his son's conscription, did try, through his friend Miller, a member of the House of Representatives, to arouse a more general support of the War of 1812 and a better organization of the American forces.

When Clinton sent Van der Kemp his satirical reply to attacks made on him by Giulian C. Verplanck, Washington Irving, and others (*An account of Abimelech Coody and other celebrated writers of New York; in a letter from a traveller to his friend in South Carolina; January, 1815*, New York, 1815), Van der Kemp wrote on April 25, 1817: "Now to the account of Abimelech Coody—How can a Western woodsman decipher the mystic rites of N. York Politicians—without you condescend to give him a clue?"

[7] Fairchild, *Autobiography*, 113–115.

[8] Fairchild, *Autobiography*, 10–13.

[9] *Ibid.*, 171–173.

[10] Bobbé, Dorothie, *De Witt Clinton*, 281, New York, 1933. Not until 1824 did Clinton's Alma Mater, Columbia, award him an honorary degree.

[11] Sereno Dwight gives an interesting sidelight on Clinton's religious devotion. Having enjoyed a long talk with Clinton on a steamboat ride from Catskill to New York, Dwight was impressed by this quality in a man whom he had previously viewed, from the angle of New England Federalism, as just a wily politician. Dwight writes of this "nobler spectacle": "a mind of this superior order humbled and penitent, trusting in the Atonement of the Redeemer for pardon and salvation, and by its own example pointing out to them [New Yorkers] the way to heaven" (Dwight to Clinton, Boston, Nov. 9, 1808).

[12] While still in Holland, in 1782, Van der Kemp published a volume of sermons; but after his arrival in America, his only scholarly printed theological publication was under the pseudonym "Candidus": Sketch of the Life of Servetus, in Letters to the Rev. Jedediah Morse, D.D., [eight letters] *The Monthly Repository of Theology and General Literature* **5**: 105–108, March; 163–168, April; 222–226, May; 277–282, June; 328–330, July; 377–387, August; 431–435, September; 525–529, November. The purpose of this work is to show Calvin's early leaning

Clinton recognized Van der Kemp's superiority, the two men had points of contact. With his letter of January 5, 1818, Van der Kemp enclosed his "Sketch" of the life of Christ.[13] "Your approbation of a 'Sketch of Divinity,'" he told Clinton on January 23, 1818, "gives me a sensible pleasure." At the same time that he sent the "Sketch," he was tempted to include some valuable theological discussions just received from England, even though he realized that this area was not Clinton's field. "My plea might have been," he wrote, "that your exalted scientific mind would not shrink to investigate if even in that labyrinth some useful truth might not be discovered." Both men, furthermore, were curiously linked with that cautious free-thinker Thomas Jefferson. Early in his career Clinton published a defense of Jefferson's orthodoxy;[14] while Van der Kemp, in his old age, arranged to have Jefferson's "Syllabus of an Estimate of the Doctrine of Jesus compared with others," published anonymously in England.[15]

In the rich cultural interchange between these two men, Van der Kemp provided the scholarship in classical and modern languages, alert to correct Clinton's Latin spellings; to provide ancient precursors of modern ideas;[16] to collect scientific data from the Oneida Lake region; and to give the tone of lofty idealism to New Yorkers' scientific researches.[17] Clinton's share was to send Van der Kemp recent volumes of transactions: those of the Literary and Philosophical Society, the New-York Historical Society, the Society for the Promotion of Agriculture and Related Arts, and to propose him as a member of these organizations.[18] Not only did Clinton furnish Van der Kemp with scientific, political, and literary pamphlets, but he also acted as a book-finding agent for his friend.[19] Extensive and carefully selected

toward Servetus' Arian position, and his cruelty in persecuting Servetus. The manuscript of these letters, together with an unpublished Life of Calvin (in which Van der Kemp recognizes Calvin's service to the law and the church, and the greatness of the *Institutes*) is in the Houghton Library, Harvard University. For a tribute to Van der Kemp the theologian, see Girelius, Charles G., Francis Adrian Van der Kemp. Unitarian Pioneer, *Unitarian Hist. Soc. Proc.* **8**: 5–17, 1950.

[13] Outlines of Memoirs respecting the person and doctrine of J.C. compiled from the S.S. and ancient writings, 3½ pp., is written in outline form, and indicates comparison of Christ's doctrine with the Jewish religion and classical philosophy, pointing out some problems of Scriptural exegesis. Van der Kemp's view appears in this sentence: "All, what [*sic*] is necessary to believe, and to be, must be so clearly revealed, as to be understood without difficulty by anyone of a sound judgment and a sincere heart." This manuscript was considered for publication by Van der Kemp's friend Dr. Joshua Toulmin in Taunton, England, but was never printed (Fairchild, *Autobiography*, 199).

[14] Grotius [De Witt Clinton], *A vindication of Thomas Jefferson: against the charges contained in a pamphlet entitled "Serious considerations,"* &c., New York, 1800.

[15] *The Monthly Repository of Theology and General Literature* **80**: 573–576, Oct. 1816. This article, showing Christ's superiority to the Jewish rabbis, was composed by Jefferson on the road to Monticello in 1803, and shown very discreetly to a few friends, including Benjamin Rush and John Adams. Jefferson was willing to have Van der Kemp publish this with his own biography of Christ (see note 13)—his only proviso was that his name be kept out of the publication (Fairchild, *Autobiography*, 170, 208–210).

[16] John Adams gives an amusing sidelight on Van der Kemp's disposition to search for facts in the distant past or remote regions of the earth. Van der Kemp asked Adams on Nov. 9, 1815: "Can you explain the spinning with a distaff? I can't conceive how. It is yet done in Siberia." Adams replied brusquely: "Every country girl in New England or New York can teach better than Hercules to spin on a distaff, which is a long conical piece of round wood, round which the well heckelled flax is bound and drawn out in thread by the thumb and finger and twisted round a spindle turned by a foot wheel. I should as soon think of sending to a satellite of Jupiter for fire to light a segar, as to Siberia to learn to spin" (n.d., Fairchild, *Autobiography*, 169).

[17] See, e.g., Van der Kemp's comment on Clinton's *Introductory discourse before the Literary and Philosophical Society on the fourth of May, 1814* New York, 1815: "May you kindle in the breasts of New-Yorkers an ardent desire, to cultivate sciences, and vie with their New England neighbors for this palm of victory" (V. d. K. to C., Oct. 22, 1816); and, on reading the first volume of the *Transactions of the Literary and Philosophical Society:* "What may not be expected from talents as those of an Hosack—or a Mitchill—or a Venerable Williamson—when they know—when they feel their Maecenas strives to press the steps of Lorenzo de Medici" (V. d. K. to C., April 25, 1817).

[18] Thanking Clinton for proposing him as a member of the Literary and Philosophical Society, Van der Kemp hesitated to join; he feared that he could contribute no useful observations, and he could ill afford the dues. Cannily, Van der Kemp suggested that the New York Society might exempt non-residents from paying dues, as the Philadelphia (American Philosophical Society) had done (V. d. K. to C., March 1, 1816). On December 27, 1816, Van der Kemp thanked Clinton for "the handsome manner" of his admittance to the Society.

[19] C. to V. d. K., Albany, April 18, 1823, promises to look for Dante's *Monarchia*. V. d. K. to C., Oct. 25, 1826, thanks Clinton for Verri's *Roman nights*, Cooper's *Last of the Mohicans*, and Shaler's *Sketches*. The first two books Van der Kemp gave to his son and daughter. He enjoyed the *Roman nights*, but apparently did not share Clinton's admiration for Cooper—in fact, Van der Kemp read very little fiction. V. d. K. to C., Jan. 23, 1818, speaks of his delight in a work of Cuvier sent by Clinton (probably *Règne animal distribué d'après son organisation*, 4 v., Paris, 1817).

though Van der Kemp's library was (it contained about 1,400 volumes at the time of his death),[20] he was the typical bibliophile in his tireless effort to fill the gaps on his shelves. While still a student at Groningen University, he had a library large and choice enough to bring him a considerable sum from its sale. At this time he planned to concentrate on works relating to Dutch politics and history. Professor Paulus Chevalier bought his collection of French deistical writers. When Van der Kemp fled to Antwerp in 1787, his wife stayed in Leyden to sell all his books on divinity, ancient and modern history, Dutch history and law. She saved for export a few chosen classics, and modern works in French, Italian, and German. After he was settled in the New World, his friends Lindsey, Luzac, Cau, and de Gyzelaer continued to send him fine editions of the classics,[21] and Paul Busti sent him Italian works from Philadelphia—for example, Pulci's *Morgante*, Palvacino's *Ricciardetto*, and the *Works* of Machiavelli, including *Mandragon*, for which Van der Kemp had been searching for thirty years.[22] Gerrit Boon bought the remnants of the library owned by the colorful Flemish *émigré* Des Wattines for Van der Kemp—later Chastellux averred that Van der Kemp had appropriated these, taking advantage of Des Wattines' distress.[23] After Van der Kemp's death in 1829, six hundred of the choice items were sold at auction in Boston.[24] In the fall of 1822, when Clinton was ousted from the office of governor, Van der Kemp's letter of regret for Clinton's defeat (November 25, 1822) includes a plaintive request about books—how many and what kind might he still expect?[25]

In forwarding copies of his own speeches to Van der Kemp, Clinton had a double purpose: to let his friend know what he was saying on historic, scientific and political issues; and to receive, with docility, Van der Kemp's criticisms, at the same time that he basked in the glow of his praise. "It will always afford me pleasure," Clinton wrote to his friend, February 21, 1816, "to hear from you and that [*sic*] if my humble efforts to promote the cause of science and the prosperity of our country, shall receive the approbation of such men as you, I shall feel amply rewarded. *Laudari a laudato vir* is surely a *laudable* ambition." On January 3, 1822, Clinton wrote to Van der Kemp from Albany: "It is not the least of my achieve-

In return, Van der Kemp sent Clinton a small silver coin, left over from the sale of his Leyden collection, for presentation to Mr. Bogert's museum (V. d. K. to C., Sept. 29, 1817). On Jan. 5, 1818, he sent Leibnitz' *Protozoa*, which Clinton promised to study carefully (C. to V. d. K., Jan. 19, 1818); and, for presentation to the New-York Historical Society, a copy of his own *Verzameling van Stukken tot die dertien Vereinigte Staeten Van Noord-America betrekkelijk*, published at Leyden, 1781, under the signature Junius Brutus, and containing a letter from John Trumbull to J. D. Van der Capellen, Aug. 31, 1779, the Constitution of Massachusetts Bay, and the speeches of John Hancock and Dr. Samuel Cooper, October, 1780. John Adams helped Van der Kemp assemble this first published presentation of American political documents to the Dutch people. In his preface, Van der Kemp drew a parallel between America and the United Provinces at that period. He expressed his pride in this work to Clinton (Jan. 5, 1818): "I defended the American cause with my pen and purse, when no distant thought lurked in my breast of visiting this country, when it required some courage to take its side against a powerful Court Party. . . ."

Van der Kemp also offered some of his rare books to Clinton's son George, at Hamilton College (V. d. K. to C., March 4, 1824); and sent the *Apologia Vanini* to Clinton for transmission to David Hosack (V. d. K. to C., Nov. 3, 1824).

[20] Howard Crosby Rice estimates Van der Kemp's library his finest contribution to American culture: "Thanks to his library, he remained a citizen of the world; and the great minds of all ages were his daily companions. With his death his library was absorbed by other libraries which were in turn enriched by it. Thus the identity of Van der Kemp's collection was lost, but the personality of the man himself is not yet wholly effaced. In the annals of American bibliophilism he deserves an honored and lasting place" (A frontier bibliophile, *Colophon* 1 (2): 234-244; 243, 1935). Rice remarks that Van der Kemp's stubborn maintenance of scholarship in the wilderness is "a quiet contradiction" of the notion that the frontier reduces all men to a low intellectual level (*ibid.*, 235).

[21] Fairchild, *Autobiography*, 16, 104. Fairchild says that in 1806 Cau sent him sixty volumes of classics, and de Gyzelaer sent Vergil, Sallust and Horace. Van der Kemp put the classics next to the hearth to keep them from the cold (*ibid.*, 157).

[22] Rice, *op. cit.*, 239-240.

[23] V. d. K. to C., March 1, 1816 (*cf.* note 106).

[24] *Catalogue of rare and valuable works, classical, theological, and miscellaneous, in the ancient and modern languages, being the library of the late Francis Adrian van der Kemp . . . to be sold on Thursday, July 15, 1830, at J. L. Cunningham's auction room, Boston*, Cambridge, 1830.

Harvard College bought about 250 of these: works on political science and theology, and some Latin classics, many printed in the Netherlands in the 1600's, including some Elzevirs (Rice, *op. cit.*, 239-240).

[25] Van der Kemp was particularly anxious about a work of Joseph Delaplaine, *Delaplaine's repository of the lives and portraits of distinguished American characters*, Philadelphia, 1815-1816, of which Clinton had sent him the first three parts. Van der Kemp was right in his fear that the work was discontinued; the projected fourth part was never issued.

ments to have acquired the esteem of good and wise men like yourself." Enclosing his speech to the Legislature, he added: "I hope that it will please you. If it does, I shall think well of it." Besides a warm general approbation of his friend's work, Van der Kemp often submitted comments page by page, sometimes offering his own observations to corroborate Clinton's speculations on natural history, and occasionally entering into a long historical and philosophical disquisition on the subject before him. He reserved the right to point out errors,[26] and once reproved Clinton for descending to vituperation of his enemies, in the midst of a scientific discourse.[27] He was especially appreciative of Clinton's Phi Beta Kappa speech at Union College, July 22, 1823.[28]

As for Van der Kemp the speaker and writer, there were only a few occasions during the years at Oneida Lake and Olden Barnveld when he emerged from the isolation of his farm to preach or to speak on some issue that deeply moved him. In 1795 he spoke at Whitestown to encourage the establishment of a Society of Agriculture.[29] Glancing wistfully here at the cultivated pleasures of cities, Van der Kemp sought compensation in the growth of crops and the study of plants and animals:

> What pleasure! what raptures we enjoy in the contemplation of a cleared, fenced acre of the first crop of corn, wheat or grass, that ever covered that spot, since the creation! Every chopped tree opens often a new scenery, every new discovered bird, reptile, plant or fossil—the anatomy of a single leaf makes our bosoms glow with thankfulness to our Maker, and opens a new field for useful—for delightful enquiries.[30]

In the systematic plan which Van der Kemp drew up for the society, Clinton was particularly struck by the suggestion of this "learned emigrant" for premiums to be given for the vanishing species of moose: $50 for the best anatomical and historical account, $60 for bringing one in alive.[31]

His "Eulogy of George Washington," delivered March 13, 1800, paid tribute to Washington the soldier, statesman, farmer, and representative of American democracy, and recalled his visit to Mount Vernon in 1788.[32] Here again Van der Kemp dramatized himself as the rustic Dutch boor, inadequate to his task.[33]

Van der Kemp's last printed theological speech was *A Wreath for the Reverend Daniel Dow* (Utica, 1806), in which he came to the defense of a young Unitarian clergyman whom Dow had attacked.

The emancipation of the Netherlands in 1814 forced Van der Kemp from retirement, to speak fervently of that freedom which he had longed for on behalf of his countrymen throughout the years

[26] For example, in acknowledging Clinton's *Introductory discourse* before the Literary and Philosophical Society of New-York, May 4, 1814 (New York, 1815, 38 pp., Notes and illustrations, pp. 39–148), Van der Kemp wrote: "No doubt I shall find much to admire—but if I casually may meet, which [i.e., something which] seems questionable, I shall with the frankness of an old soldier, and the liberty of a member of the literary commonwealth communicate my sentiments upon it" (V. d. K. to C., Jan. 8, 1816).

[27] "May I take the liberty to ask, if the President of the Philosophical Society ought to have condescended—to take notice of the slanders in the *Quarterly Review*?" (*ibid.*).

The article in question, a review of *Inchiquin, the Jesuit's letters . . . by some unknown foreigner*, New York, 1810, reputed to be by Robert Southey, was a snide attack on American civilization, with politics and law the chief targets (*Quarterly Review* **10**: 494–539, Jan., 1814). Clinton was especially irritated by the criticism of Jefferson, Franklin and his friend Thomas Addis Emmet (*Introductory discourse*, note 1, pp. 39–40).

[28] This address, one of Clinton's best (Phi Beta Kappa, William W. Campbell, *The life and writings of De Witt Clinton*, pp. 329–363, New York, 1849), emphasizes the importance of knowledge in a free government, praising individuals who contributed to law, science and the arts; it dwells on his favorite theme of the interaction between agriculture and commerce, and the importance of navigation as a link between the two; and it glows with his patriotism for New York State. It includes a tribute to the translations of the Dutch Records by "the erudite Van der Kemp" (p. 357).

[29] *Speech of Fr. Adr. Van der Kemp, at a meeting the first of June, one thousand, seven hundred and ninety-five, at Whitestown for the institution of a society of agriculture*, Whitestown, 1795.

[30] *Ibid.*, 6.

[31] *Introductory discourse, May 4, 1814*, 27–28. Here Van der Kemp struck a chord of sympathy with Clinton's ardent desire to preserve New York's antiquities.

[32] Martha Washington appreciated this eulogy (Martha Washington to Van der Kemp, March 13, 1800, Buffalo Historical Society). A copy was published in Holland: *Lofrede op G.W., te Olden barneveld, der 22sten van Sprokkelmaand 1800, in Oneida district . . . in de Engelsche taale uitgesprooken, door F. v.d. Kemp . . .* (Amsterdam, 1800, with Preface by translator Dr. L. Bendikson). The first English publication was prefixed to *Washington eulogies, a checklist of eulogies and funeral orations on the death of George Washington December, 1799–February, 1800*, compiled by Margaret Bingham Stillwell, New York, 1916.

[33] "How discordant M.H. [my hearers] are the woods with the purity of classical elegance! I must hew with an axe the requested resemblance, and shall consider myself fortunate, you indulgent, if you recognise in my rude strokes any likeness of your beloved Washington" (Stillwell, *Washington eulogies*, 12).

as an *émigré*.[34] In an undated letter Clinton wrote of this appreciatively:

> I beg you to accept my best thanks for your interesting oration on the emancipation of the Dutch. An event of so much importance to the cause of national independence, involving the destinies of a people so intimately connected with many of us by consanguinity and commemorated in such an able and elegant manner must make the deepest impression upon any intelligent and cultivated mind.

These few occasional expressions were not enough to satisfy the Van der Kemp who, as a young man in the Netherlands, had been not only an eloquent preacher but also an effective political pamphleteer and publisher.[35] Aware of his imperfect command over the English language (although his use of American idiom gradually improved, it still retained a Dutch flavor), he turned to Clinton for help in polishing his treasured manuscript essays for publication. With his first letter to Clinton (September 14, 1812), reminding him of their meeting many years before, and of the assistance given the Van der Kemps by George Clinton, Francis Adrian enclosed what he then called "Sketch of a Desirable Work,"[36] asking for Clinton's remarks on it.

> Can you, who so boldly planned, to enrich New-York State with a magnificent canal-navigation—whose execution alone would immortalize a Weston—could you Sir! be induced—to embody this skeleton—and inspire it with life. I too should have well deserved of my country—though I only carried the raw materials together.

The second essay sent to Clinton was "Researches on Various Parts of Buffon's and Jefferson's Theories in Natural History," a 270-page manuscript, begun in 1800 at the suggestion of Chancellor Livingston,[37] and revised in 1811. Praising Buffon's and Jefferson's significant contribution to natural history, Van der Kemp finds flaws in both writers. He exonerates Leibnitz of Buffon's adverse criticism, and studs the essay with quotations and elaborate footnotes. His chief original contribution is his pious Unitarian belief in an all-wise, consistent Deity.

The third essay submitted for Clinton's criticism, on January 8, 1816, was "Letters from Francis Adrian Van der Kemp to Col. Adam G. Mappa, on a Tour through a part of the Western District of New York, in 1792," originally written in Dutch for his own family as well as for Mappa, later translated into English.[38] The fourth was a treatise on the use of copper by the ancient Greeks, which Clinton acknowledged March 24, 1817, promising an early comment.[39] On January 23, 1818, Van der Kemp proposed to send a fifth essay, "Sketch of the Achaic Republic," recently returned to him from England. (Joshua Toulmin, who had considered it for publication, died in 1815.) Confident of

[34] *An oration delivered on the 11th of March, 1814, at Utica, commemorative of the emancipation of the Dutch from French tyranny*, Utica, 1815. The strong emotion of this speech shows that Van der Kemp's friend Peter Vreede was wrong about Van der Kemp's interest in the Dutch struggle. Objecting to the interception of Van der Kemp's letters to him from Esopus (now Kingston), where Van der Kemp first settled, Vreede, in Brabant at the time of the first Revolution there, wrote: "Deliver me those letters from my friend! Good God! Burgomaster! Esopus is a village three thousand miles away in America, and my friend not even thinks of your miserable Revolution" (quoted in V. d. K. to John Adams, Aug. 3, 1808, Fairchild, *Autobiography*, 117).

[35] See, e.g., note 19, on Van der Kemp's *Verzameling*.

[36] This sketch is an outline of historical events, political and philosophical writings, which Van der Kemp hoped would be enlarged by someone into a volume on the revolutionary spirit. It was eventually published as Sketch of the Moral and Physical Causes of the Revolutionary troubles during the XVIII century and its probable consequences in both hemispheres, in the Boston *General Repository and Review* **4**: 390–397, Oct. 1813.

Van der Kemp's letter of Sept. 14, 1812, was marked by Clinton "to be answered." Van der Kemp's letter of April 25, 1817, telling of the publication of this work and its reprinting in Europe, includes his regret that Clinton never acknowledged the manuscript (nor did he ever return it; it is still in his Papers).

[37] V. d. K. to C., Oct. 27, 1815. This manuscript, written as Preliminary Letter and Twelve Letters, to Gerrit Boon, now in the Buffalo Historical Society, is still unpublished. Jefferson read it in its embryo state, and encouraged Van der Kemp to continue working on it. Charles Eliot, Van der Kemp's Boston friend, offered to correct the language, but died before he could finish it. A letter from John Adams to Van der Kemp, Quincy, March 3, 1804, shows that this early Van der Kemp was seeking Adams' assistance in persuading the Academy of Arts and Sciences to publish the work. Adams offered to nudge the reading committee, but disclaimed any knowledge of natural history, considered Buffon a despicable materialist, and asked Van der Kemp not to dedicate the work to him (John F. Seymour, *Centennial address delivered at Trenton, New York, with letters from Francis Adrian Van der Kemp, written in 1792, and other documents relating to the first settlement of Trenton and central New York*, 21–22, Utica, 1877. *Cf.* notes 46–47).

[38] Printed as Appendix to Seymour, *Centennial address*, 48–128. For Clinton's reaction to this manuscript, see note 99. Van der Kemp asked Clinton to send these "Letters" on to John Adams, who wrote of them: "At the hazard of the little vision that is left, I have read your travels in the wilderness" (Quincy, March 2, 1816, Fairchild, *Autobiography*, 176).

[39] This manuscript is not in the Clinton Papers. Presumably it was returned, but I have not found any comment of Clinton's on it.

its historical truth, Van der Kemp was as usual apprehensive about the language. Apologizing for not copying it, he said: "Now it solicits for admission in a tattered dress." Although he continued to cherish great hopes for this sketch, it was never published.[40]

On occasion Van der Kemp felt willing to turn over his papers to Clinton for his own use, not even asking for any acknowledgment. "*Meum opusque tuum est*," he wrote to Clinton, December 27, 1816. After commenting on Boudinot's *Star in the West*,[41] he asked: "Will you permit me the offer—if any facts were recorded in my Researches—that they may be considered as your own—and make use of them?"[42]

But the urge for personal—even if posthumous—fame was still strong. In July, 1822, Van der Kemp wrote that he was anxious to get back his papers, with Clinton's comments, so that he might produce something for his successors. And on September 29, 1823, he again urged Clinton to go over his manuscripts.

> I have resolved to review the whole—and try, if I cannot correct errors and increase their worth—as my days cannot be many—and it is time to bring all my little trifles in somewhat better order—before I enter that long voyage from which no traveler returns—yet—Clinton! we do not live without hope.[43]

Clinton never did give Van der Kemp the kind of criticism that his friend demanded. Deeply interested in Van der Kemp's mind and scholarship, Clinton could not bear to return the manuscripts with only a cursory examination; but official duties, support of numerous organizations, family cares and a voluminous correspondence kept him so occupied that he could not polish his own essays, let alone find time for the scrupulous editing that Van der Kemp's work required.

Nevertheless, the intellectual companionship between the two men was so rewarding that it might be termed cross-fertilization. Prominent in the correspondence is their keen mutual interest in natural history. Clinton wrote to Van der Kemp on November 20, 1815, congratulating him on his study of the ample and interesting field of inquiry into the flora and fauna of his part of the country.[44] Reading Van der Kemp's "Letters on a Tour through . . . the Western District of New York," Clinton wrote on February 21, 1816, of the stimulus he felt in the contrast between the western (today termed central) district in that early period and its cultivated state. Van der Kemp, in turn, sent on January 8, 1816, a detailed comment on Clinton's *Introductory Discourse* of May 4, 1814. He could confirm Clinton's observations concerning the dried-up lakes of central New York[45]—in fact, Van der Kemp wrote, he was then sitting on one.

Ever since Buffon published his depreciating account of American plants and animals, as compared with those in Europe,[46] and Jefferson rose to the defense of American species,[47] it was a strong concern of American scientists to estab-

[40] Mentioning the "Sketch" in his letter of December 6, 1817, Van der Kemp spoke of his earlier hope of publishing it in Europe, in one of the European languages. In America, he feared, its publication might be followed by a cry of "BREKEKEX-COAX-COAX." Van der Kemp wrote to Clinton, Jan. 26, 1818, to say that he was sending the "Sketch" to Albany by Mrs. Morris Miller. Fairchild indicates that this was still in Clinton's possession at the time that Van der Kemp was writing his *Autobiography* (*op. cit.*, 199), but it must some time have been returned. The only essays of Van der Kemp now in the Clinton Papers are the above-mentioned "Sketch" of the life of Christ, the revolutionary "Sketch," "Symposium uticense," a *jeu d'esprit* written Jan. 1811, containing some serious statements on Dutch-American relations; and the tribute to Clinton as originator of the canal (see note 100). The last two Van der Kemp did not ask to have returned.

[41] Elias Boudinot, *A star in the west* . . . , Trenton, N. J., 1816, develops the theory that the Indians were descendants of a lost tribe of Israel. Both Clinton and Van der Kemp accepted the theory (see V. d. K. to C., Feb. 12, 1817; Clinton, "The Iroquois," Campbell, *Life and writings*, 246, and a three-page draft for an article on the subject (Clinton Papers, 21: 206–209).

[42] V. d. K. to C., April 25, 1817.

[43] Van der Kemp of course did not rely wholly on Clinton. The essays sent to Clinton were historical and scientific; his theological essays were entrusted to such friends as Jedediah Morse of Boston and Joshua Toulmin in Taunton, England (for an exception, see note 13).

[44] This was occasioned by Van der Kemp's "Researches on the various parts of natural history in the theories of Buffon and Jefferson" (*cf.* note 37).

[45] *Cf.* note 26. The *Discourse* contained Clinton's hypothesis of the recession of Lake Erie and the former existence of a great lake above Little Falls, connected with Oneida Lake (note 7, pp. 51–54). Clinton had given as evidence for this drying up of lakes in central New York the discovery of a large tree at Rome when William Colbreath dug twelve feet down for a well. Van der Kemp, an eye witness of Colonel Colbreath's digging, confirmed this observation, and added his observation of a squared log, charcoal and ashes taken from the cuttings in Wood Creek. Enthusiastic about Clinton's demand for a more systematic study of the geology of New York State (*Discourse*, 22–25), Van der Kemp suggested dividing the state into five sections, and making a detailed survey (V. d. K. to C., Jan. 5, 1818).

[46] George Louis Leclerc Buffon, Comte de, *Histoire naturelle, générale et particulière*, 44 v., Paris, 1749–1804.

[47] Thomas Jefferson, *Notes on the state of Virginia* . . . , Paris, 1784–1785.

lish as many native species as possible—but also to be scrupulous in distinguishing indigenous species from imports. In this field Van der Kemp was glad to confirm Clinton's guess that the gray and white turkeys were of European import; the brown, indigenous. The European types had been observed in Madison and Dutchess County.[48] Along the same line, Clinton urged Van der Kemp to send him the petrified honeycomb and bees which Judge P. Smith of Madison County had shown Van der Kemp. If this petrified insect could be identified as the real honey bee, it would help to settle the moot question of the honey bee's American nativity.[49] In this same field, Van der Kemp could assist Clinton by his knowledge of Dutch plants, animals, and fish, comparing them with American species. The Dutch *aart-aackers*, Van der Kemp at first averred, were the same as the Yankee artichokes; on further examination, he found differences between them.[50]

However delighted Clinton was to add Van der Kemp to his corps of scientific researchers (which included professors, army generals, ships' captains, and engineers), he realized that this friend was less well read in modern scientific theory than in the classics.[51] Clinton tried to bring Van der Kemp abreast of modern scientific work, not only through the volumes of transactions of the New York and Connecticut societies, and innumerable pamphlets, but also through significant books. On January 31, 1822, he sent Van der Kemp a copy of Amos Eaton's *Geological and Agricultural Survey of Rensselaer County* (Albany, 1822), which his friend acknowledged with appreciation, stating that it might well be imitated by every New York county. In Van der Kemp's view, the work would have been still more valuable if it had included information on orchards and cattle feeding.[52] On July 13, 1824, Clinton sent a copy of Benjamin Silliman's *Remarks, made on a short tour, between Hartford and Quebec, in the autumn of 1819 . . .* (New Haven, 1820) an entertaining book, but also important for its geological observations. Van der Kemp's enjoyment of this work was tempered by criticism. Although he had a favorable opinion of Silliman (earlier he had done some classical research for the Yale professor), this book did not enhance that opinion. Van der Kemp felt that it was too patently directed toward popular appeal; he criticized the language, especially the term *trap* as applied to rock (here Van der Kemp did not understand the geological sense of the term); and he said, with some justice, that at least one print of New York or Quebec should have been included (both illustrations are New England views).[53] Yet, even though Van der Kemp insisted on his right to correct the scientific *savants*, he was quite aware of his own shortcomings, and tried to fill in the gaps. In botanical and zoological knowledge he deferred to Adam Mappa, and sent, through Clinton, specific queries to Samuel Mitchill and David Hosack.[54] No one could call Van der Kemp a prosperous farmer—his garden and livestock barely sufficed to feed his family—but he had an interest in different varieties of trees, plants, and vegetables. On May

[48] V. d. K. to C., Jan. 8, 1816. *Cf. Discourse*, note 13, pp. 84–89.

[49] C. to V. d. K., Albany, Feb. 21, 1816. In his letter of March 1, 1816, Van der Kemp promised to try to get the petrified honeycomb—but there is no evidence that he succeeded. Clinton's *Introductory discourse* shows how anxious he was to prove the honey bee of North American origin (note 30, pp. 121–124). His own observation of the numerous bees in New York's wild forests was good evidence for the case.

[50] V. d. K. to C., Jan. 8, 1816. Clinton's *Introductory discourse* described the *Glycine apios* of Linnaeus, *Apios tuberosa* of Moenchhausen and Pusch (wild potato) as a favorite food of the Indians, and nearly as good as the common potato. He also noted that the *Helianthus tuberosus*, or Jerusalem artichoke, growing spontaneously over the country, ought to be cultivated (note 32, pp. 127–128). He pursued this matter in a letter to Van der Kemp, Jan. 31, 1822, asking for specimens of the two kinds of wild potato growing near Oneida Lake, said to flower from July to September. Van der Kemp replied on Feb. 11, 1822, that Adam Mappa would give him further information on the *Helianthus* on West Canada Creek, on his return from Philadelphia. Both American kinds, he said, were distinct from each other, and from the *aart-aacker* of the Dutch. The fruit of the *Helianthus* was insipid, used by the Yankees in pickles, while that of the *aart-aacker* was of firm texture, and served boiled or roasted, like the Spanish chestnut.

[51] Clinton wrote in the Hibernicus *Letters* on the occasion of their meeting in September, 1820: "I could perceive no deficiency, except in his not being perfectly acquainted with the modern discoveries in natural science, which arises in a great degree from his sequestered life" (*Letters*, 219).

[52] V. d. K. to C., Feb. 11, 1822. Although Eaton unquestionably contributed to the science of geology, especially of New York State, he came in for sharper criticism than Van der Kemp's (see, e.g., the review of his *Geological text-book*, Albany, 1830, *North American Review* 32: 471–490, April, 1831).

[53] V. d. K. to C., July 28, 1824.

[54] "Tell me once," he wrote to Clinton on April 21, 1823, "who of the moderns gives the best account of the *Fungi*—the *Champignons*—and illustrated [*sic*] the discoveries of microscopical observation—I know several French *savants* did so—but who of them—with the greatest accuracy—and who the first?"

3, 1822, he sent Clinton a little carrot seed, used in Holland for feeding cattle. If properly grown, in strong soil, it would yield carrots from nine to ten inches. On May 5, 1823, he asked Clinton to send him two Long Island Pepins, to accompany the native apple trees given him by Jonas Platt, and two flower bulbs for his daughter, from Clinton's country home.[55]

Of all the varied divisions of natural history, both Clinton and Van der Kemp were especially fascinated by fish: their species, habits and habitats, and—to these gourmets, of equal importance—their methods of cooking.[56] Clinton wrote two essays on fish,[57] and was delighted when he had a fish named after him. His letter of February 21, 1816, to Van der Kemp, was full of fish. He wanted a careful description of a green-colored fish, of exquisite taste, which Van der Kemp had mentioned.[58] What fish did Van der Kemp mean by the *Tzitzeb*?[59] Should not Van der Kemp write a Memoir on the American salmon vs. the Dutch—the same species, or different? The Society for the Promotion of Useful Arts would surely be interested in a report on the best method of collecting, preparing, and preserving them.

Van der Kemp replied enthusiastically on April 25, 1817, to the request for information about salmon. Praising Dr. Samuel Mitchill for his desire to preserve the salmon in New York waters, he corrected Dr. Mitchill on his Dutch terms.[60] *Salm*, or *salempje*, Dr. Mitchill had written, was used by the Dutch to signify salmon, or, in loose conversation, trout. *Salm*, Van der Kemp averred, meant salmon; *salempje*, a little salmon—neither would be used to designate trout (for this, the Dutch would use the French *truit*). Whether the salmon and trout were different genera, he could not be sure without examining the gills and scales of both—in his view, a more reliable method than examining fins. He sent Clinton a lengthy description, obtained from his Dutch friend Herbag, of the manner of catching, curing, and smoking salmon in Holland. If salmon was to be eaten fresh, it was preferable in the latter part of the summer, taken to the market alive, and then cut. This lean salmon was distinguished from the fat salmon of fall and winter. "The salmon well smoked," he said, "would become a gold mine for that part of the state [Fish creek and Salmon river]." His criticism of the salmon smoked on Scriba's patent was that they salted it too much and smoked it too hard. Van der Kemp preferred the Dutch method of taking the salmon in seines, to the New York way of catching it with rod and line from canoes or boats. In Holland it was smoked throughout the year, but the winter salmon was the best for this process.[61]

With his letter of April 25, 1817, Van der Kemp enclosed delicate drawings of sole and plaice, done by Adam Mappa, to illustrate the difference between these species, which Dr. Mitchill had confused. "I am no Zoilus—" Van der Kemp wrote, "but a cobbler may sometimes correct Appelles."

Closely related to fish was the so-called "Lake Blossom" on Oneida Lake. Clinton wanted to know whether this was made up of insects, or

[55] With his letter of Jan. —, 1818, Van der Kemp sent Clinton some notes on the cultivation of poppies, and a Dutch report on how to treat lameness in sheep, taken from the *Reports* of the Society of Agriculture at Amsterdam, **3**: 45–52.

[56] Van der Kemp took special pride in his wife's preparation of fish, and Clinton enjoyed no meals so much on his western tours as the dinners of Oneida salmon and garden vegetables served by Barthe Van der Kemp. Commenting on Charles B. Vignoles' *Observations on the Floridas* . . . , New York, 1823, Van der Kemp wanted to know if hops were used as a vegetable in New York, as in Europe, especially in spring. Why, he asked, did not the Floridians send turtle soup to New York as well as to London, so that Clinton could have this with his favorite Burgundy? (V. d. K. to C., May 5, 1823).

[57] *Remarks on the fishes of the western waters of the state of New York in a letter to Samuel L. Mitchill, M.D.* . . . , New York, Feb. 1, 1815; *Account of the Salmo Otsego, or the Otsego Bass: in a letter to Dr. John W. Francis* . . . , New York, 1822.

[58] Van der Kemp replied to this on March 1, 1816, describing the only specimen he had seen, caught while in Mappa's company: grass-green, here and there a deeper hue, its meat weak, though fine—perhaps it was its sickly season. He would not hazard an opinion on the species until he could find another specimen. Clinton pursued the question six years later, asking Van der Kemp to get him one if possible (C. to V. d. K., Jan. 3, 1822).

[59] Van der Kemp described the *Tzitzeb* (his spelling) as a small fish with brownish scales, the meat weak, abundant in the lake and all creeks emptying into the Mohawk, seldom found in the same spot with trout. On April 25, 1817, Van der Kemp admitted that he had spelled it wrong: the fish he was describing was the humble *chub*.

[60] *Report, in part, of Samuel L. Mitchill, M.D.,* . . . *on the fishes of New-York* . . . , New York, Jan. 1, 1814.

[61] Another copy of this report, accompanying a letter of Nov. 25–Dec. —, 1822, is in the Clinton Papers **24**: 65. In this letter Van der Kemp suggested that the Dutch method of smoking salmon, if directed to the New York and Canadian markets, already receiving Oneida and Ontario whitefish, pickerel, trout and salted salmon, might make a fortune for some New Yorkers, as it had done for Schoonhoven in Holland.

insects and their ova.[62] Van der Kemp replied that he believed them to be chiefly insects, the same as the exuviae or teguments of the *Hasts* which he had often seen floating on Dutch rivers; he was not so positive about their ova. They formed a palatable food for fish:

> The teguments called by the Dutch *hembdjes* (little shirts) present the perfect resemblance of the insect —I have often seen them at the instant when the insect lit it, and remained for a few moments attached to the same. Their immense multitude occasions at the Oneyda Lake a disagreeable smell on the shore, for a few days. They are here in smaller quantities and leave their teguments in the leaves of shrubs and panes of windows—near the creeks.[63]

One strange aspect of natural history which particularly absorbed Van der Kemp's attention was the curious varieties of serpents (a term also used of the whale). "Have you observed in Southey's *Roderick*, in the notes," he asked Clinton on March 1, 1816, "the mention of serpents with two heads?" On October 22, 1816, he repeated the request. Clinton's letters reveal no answer to this query, which he may have answered in conversation.[64] Having read Humphrey's and the Linnaean Society's reports on the great serpent, Van der Kemp wrote of his interest in this species (December 6, 1817), and averred that a specimen of the great would have to be taken, before determining whether it was the same species as the small. Aldrovandus, he found, mentioned some points of resemblance.[65] Having heard of an account by John Pintard of "fascinating serpents," Van der Kemp wrote to Clinton on September 29, 1817, to procure it for him. Having read Benjamin Barton's debunking account of this topic, he was inclined to disbelief—but still curious.[66]

When the study of natural history—especially geology and astronomy—threatened the validity of the Old Testament revelation, as it did, even thus early in the nineteenth century, both Clinton and Van der Kemp met the challenge with sublime confidence. Referring to a conversation with Clinton at Dr. Hosack's in New York, when Van der Kemp spoke of observations on sunspots from Burton's *Anatomy of Melancholy*, he wrote to Clinton on December 6, 1817, to explain his motivation:

> I did so to rouse so many scientific men—to the heroic achievement of ascertaining their true nature —Perhaps this glory is preserved for an American philosopher. The sun's atmosphere—and to its irregular motions—perhaps—the appearing and the disappearing of the Spots may be owing. Could they [be] ascertained to be planets—then we might have made a step further to the knowledge of the sun—and the learned Dr. Swinden—if instructed that the apparent fiery brilliancy is originating from the effluvia of the atmosphere—might have chosen one of these black Planetarian spots for his hell—in lieu of the sun; that noble spot in the immense expanse of the Almighty's creation—in brilliant effulgency existing—when our little globe was renovated again and again—and that to become a receptacle for a few inhabitants of this sphere!

Finding a few difficulties in the theory of the earth as explained by Dr. Derby's *Memoir*,[67] Van der Kemp still believed that a "vast mass" was enveloped in "Cimmerian darkness":

> The Mosaic account does not seem to insinuate any point further than the renovation of this our planet —by the Almighty Creator of the Universe—Its

[62] C. to V. d. K., Albany, Feb. 21, 1816.

[63] V. d. K. to C., March 1, 1816.

In the opinion of Dr. A. W. Eipper, Department of Conservation, New York State College of Agriculture, Cornell, these surface scums on Oneida Lake are composed of both plant and animal material, with plant material, chiefly single-celled algae or "phytoplankton" predominating. Dr. Eipper indicates that closer analyses of these scums still need to be made. *Cf.* Frank C. Baker, *The productivity of invertebrate fish food on the bottom of Oneida Lake*, New York State College of Forestry at Syracuse, Technical Publication #9, 1918.

[64] Clinton had received from Samuel Mitchill a full report on the two-headed North American serpent, together with a specimen given to Mitchill by John Bogert. Having seen four specimens of this serpent (one in Edinburgh, one in Quebec, one in Washington, shown by Jefferson, besides the one sent to Clinton), Mitchill concluded that the species was peculiar to America, and he was inclined to believe that they were not monsters, but a regular natural production. He contradicted the vulgar notion that the two heads issued from opposite ends of the backbone (Samuel Mitchill to C., New York, Nov. 21, 1814).

[65] Van der Kemp the scholar was annoyed because he could not find the reference he had to Pliny on this point; and he added—doubtless as a hint to Clinton—that he did not own Pliny's *Historia animalium*. He also mentioned the report in the *Manuel du naturaliste*, dédié a Buffon, that the aquatic serpent was common in the North Sea.

[66] Van der Kemp mentioned similar accounts by Montaigne, Pompeius Shela and Aulus Gellius. Van der Kemp's interest in astronomy also appears in his letter of Nov. 3, 1824, in which he suggested that Clinton, as the "Grand-Master Mason," build on the heights of New York "a splendid observatory," superior to the most vaunted in Europe, and finish it by 1825.

[67] Van der Kemp's letter of September 29, 1817, had asked Clinton for this work: "As I indulged—long since—in the same reverie—could its plausibility be rendered nearer approaching truth—it would not only spread a light on geology—but explain many points of Divinity. It would become an easy task, to develop the history of good and bad angels &c."

vivifying powers were exhausted—the whole globe reduced to a chaotic mass—from which state it was called again to a new life—when the solid parts at length were reunited—the waters—subsided—collected—cleansed—the atmosphere renewed, the earth once more was illumed by the sun—and plants and beasts and men created—then it was—"that the praises of the Almighty were sung in the Highest Heavens"—then it was—that peace and blessedness were anew proclaimed to the new inhabitants of this earth—While the myriads of mortals adored in solemn chants—the good will—and all powerful wisdom of the Father of the Universe. Then the morning stars sang together in a choir, and all the Sons of God shouted for joy. You perceive by this, Sir! that it smells [of] no heresy—it is pure orthodoxy—and I am confident, if Cuvier—and other scientific men—possessing such transcendent talents—were equally eminent orientalists—they would from the *Mosaic accounts* and the sublime outlines in Fabritius . . . have brought light forward from darkness.[68]

Inspired by Leibnitz, Van der Kemp continued his comments on the destruction and re-formation of the earth in his letter of January 5, 1818. Both opposing camps of geologists, the Neptunists, followers of Werner, who believed the oceans the chief agents of change, and the Plutonists or Vulcanists, followers of Hutton, who assigned the chief power to volcanic action, seemed to Van der Kemp to be founded in truth. "It requires," he averred, "only an unprejudiced bold mind—to bring both schemes in a perfect unison—a gigantic undertaking—nevertheless—worthy the exertions of one daring to aim at the first station of scientific eminence." Since neither Werner nor Hutton conceived a theory of the earth's origin in any way approaching the currently accepted view, that the earth was formed at the same time as the sun by the aggregation of cool interstellar gas and dust particles, Van der Kemp's prophecy of a reconciliation between the two nineteenth-century theories has not been fulfilled. On the other hand, although water is now recognized as the chief re-fashioner of the earth's surface, volcanoes have also played a part; in this sense, neither the Neptunist nor the Vulcanist theory has become entirely outmoded. And, one might add, that greater destructive power than fire or water, discovered by the "bold minds" of the twentieth century, renders a fresh significance to Van der Kemp's speculations about the future destruction of our planet.

Clinton's reply of January 19, 1818, spoke more briefly but with an assurance equal to Van der Kemp's:

> Do not all the phenomena which we witness proclaim that this world is the wreck of a former one. If the Mosaic account be considered as necessary to a revelation, it is wonderfully accordant with all the appearances of Geology—and with the most approved modern theories; all at first was Chaos—then there was a new organization or arrangement of matter—then plants—animals—lastly man—and then a partial destruction by a deluge. If the works of Werner and Cuvier and all the geological facts which the industry of man has discovered—had been spread before Moses when he wrote, he could not have been more accurate in his cosmogony.

On March 20, 1823, Van der Kemp asked Clinton for information on granite petrifactions, seeking further light on the theory of creation:

> Should there not exist—in the manner of our solar system—upon a more magnificent plan a general solar system—comprehending all the fixed stars in the universe? If we are immortal—destined once for higher action—may not the same Almighty, wise and good Being preside in a similar manner [over] the whole animal creation for a continual progress?

Writing of his experiments in "taming" such wildflowers as the hyacinth and the rose (he succeeded in domesticating one wild rose in his garden, which he named "the Olden Barneveld," and he was hoping to grow black roses), Van der Kemp added:

> I doubt not or [*sic*] the wild roses, if cultivated, might in time arrive at a state of higher perfection—and may not the same be supposed of the whole animal creation—in the present already the worm changes in a butterfly and men may become angelic spirits.[69]

[68] Because of his realization of the importance of language for the study of history as well as of science and theology, Van der Kemp was delighted to read a notice in the *North American Review* of Champollion's discovery of the Rosetta Stone: *Sur L'écriture dêmotique; précis du système hiêroglyphique des anciens Egyptiens*, 1824. His next-to-last letter to Clinton, January 29, 1828, requested a copy of this book.

[69] It is significant that Benjamin Silliman, Senior, more keenly aware than such amateurs as Clinton and Van der Kemp of the embarrassments which the facts of geology caused in a literal belief of the Old Testament, nevertheless maintained his trust that the two could be reconciled. "The Bible," he wrote, "is not a book of physical science, and its allusions to physical subjects are necessarily adapted to common apprehensions. Still, the Creation and the deluge, although they have a momentous moral bearing, were, in *their nature*, entirely physical operations." Ingeniously, Silliman presupposed a period of time before the six days began, and drew up a Table of Coincidences between the order of events as described in *Genesis* and that unfolded by geological investigation. He also warned scientists not to try discussing this touchy problem with the average man ("Suggestions Relative to the Philosophy of Geology as deduced from the facts and to the consistency of both the facts and

This welcoming of scientific inquiry, however, did not mean, in either Clinton's or Van der Kemp's mind, the breaking down of all barriers to theological speculation. Even though Clinton had wide tolerance for religious views different from his own (through his efforts all property and voting restrictions in New York were removed from those of the Roman Catholic faith,[70] he spoke in violent denunciation of an anonymous work, *Ecce Homo*,[71] which Van der Kemp had asked him about:[72]

> *Ecce Homo* is a book highly blasphemous. The Trinitarians believe in the divinity of the person, as well as the mission of Christ, the Unitarians only in the divinity of the mission—both creeds ascribe the utmost purity to Jesus, and consider him with the highest veneration; but *Ecce Homo* assails his moral character, and treats him as an impostor. The book is not for sale and I cannot ask the author for a perusal. It would be indirect encouragement.[73]

Van der Kemp, in reply, quite agreed with Clinton. He suspected that the writer of *Ecce Homo* had the same fault as the French deists: personal vanity.

> We may humbly inquire, always with due reverence to a First Cause—a Being—all wise and good—and I cannot see—while it is as impossible to penetrate as to develop his wonderful creation—yet we can unravel a part—more so when it increases our ideas of his almighty power and wisdom—perhaps at another period of our existence, we may stand amazed at our present hallucinations—and even then we shall see only in part.[74]

Not only in their confidence that science could assist religion, but in their general religious views, these two men were surprisingly harmonious, considering their difference on the subject of the Trinity. On December 6, 1817, Van der Kemp wrote to congratulate Clinton on his proclamation, as governor of the state, of a public Thanksgiving.[75] Van der Kemp would have liked to preach in one of the New York City churches on that day. However glad to have his friend's approval, Clinton must have winced when he read the topic contemplated by Van der Kemp: the dangers of a military government in general, and specifically, the evil of fortifying New York City.[76] Van der Kemp spoke again of the Thanksgiving proclamation, on December 26, 1821, as one of "those gigantic monuments" raised by Clinton, "which Party Spirit cannot shake, and on which Envy's gnashing teeth shall be broken." At the moment this act of Clinton overshadowed his services to the arts, science, and agriculture:

> that noble custom of our Dutch forefathers here—of thanking God annually for his undeserved blessing—and imploring his mercy for our transgressions—this cannot become again obsolete, as no successor can be so unprincipled, that he would risk his own credit, by neglecting this dutiful homage.[77]

Van der Kemp showed his own high hopes for the future position of religion in American life in his letter to Clinton of October 22, 1816, where he mentioned Bishop Berkeley's "Westward the star of Empire takes its way," and a passage from a poem of George Herbert, mentioned by Bishop Tillotson:

> Religion stands on tiptoe in our land
> Ready to press to the American Strand
> When Seine shall swallow Tiber, and the Thames
> By letting in them both, pollute her Streams,
> Then shall Religion to America flee;
> They have their home of Gospel, even as we.

If, as these men believed, natural history in its upward aspirations could throw light on the all-important realm of religion, it was no less useful at its base, as assistant to technocracy. Van der Kemp was less interested than Clinton in the multifarious inventions of the day—from steamboats to excavating machines—[78] but in the great engi-

theory of this science with Sacred History," Appendix, Robert Bakewell, *Geology*, 78, 84–5, 103, 116, New Haven, 1839).

[70] Bobbé, *De Witt Clinton*, 121.

[71] *Ecce Homo! or a critical inquiry into the history of Jesus Christ*, translated from the French, by George Houston, London, 1799; 2nd ed., London, 1813. Clinton evidently knew the author, Paul Henri Thiry, Baron d'Holbach, but did not disclose his name.

[72] V. d. K. to C., March 20, 1823.

[73] C. to V. d. K., Albany, April 8, 1823, in Seymour, *Centennial address*, 20.

[74] V. d. K. to C., April 21, 1823.

[75] This praise was welcome to Clinton, as was that of Thomas Robbins, who wrote from East Windsor, Connecticut, October 10, 1818, to congratulate Clinton on this step, which the governors of some other states were following. Elijah Hawley sent a somewhat qualified approval from Ridgeway, October 27, 1817, mentioning the improper use made of the festival in some New England states. The Quaker leader Elias Hicks, with the typical Friend's objection to special days of worship as well as to any interference of government with religion, sent his strong letter of disapproval on November 4, 1818.

[76] For Clinton's work in fortifying New York City during the War of 1812, see note 83.

[77] V. d. K. to C., Dec. 26, 1821.

[78] Van der Kemp did supply Clinton with the name of the chemist (Murray) who made in Holland a blow pipe with a special safety-valve (Van der Kemp called it *a valve of security*), that Clinton had procured from London (C. to V. d. K., Jan. 6, 1817; V. d. K. to C., Sept. 29, 1817). And on July 13, 1824 Clinton promised

neering project of Clinton's life, the Grand Canal, Van der Kemp was an enthusiastic supporter. With Clinton at the helm, all the resources of pure science were laid under contribution for the planning and execution of this practical undertaking. A knowledge of geology was essential to estimate the difficulty of excavation, to discover native limestone—to be used in making native cement, and to estimate the extent of salt mines in the vicinity of Syracuse (the tax on salt was to become one of the chief means of paying construction expense). Geographical and topographical knowledge was important in surveying creeks and rivers to be used in charting feeders for the canal. Even botany was significant.[79]

The main problems, however, were technological: how to find men, skill, and machines to carry out a project vaster than any yet attempted in the western hemisphere; and political—how to persuade the Legislature and the people that the canal could be dug without bankrupting the State of New York. In this enterprise, to which Clinton gave all his mind, political sagacity, and unremitting attention from 1816 to 1826, he was not only encouraged, but actually assisted by Van der Kemp.

Van der Kemp was well aware of Clinton's agency in the first attempt at a state-supported canal (1810–1811), which was thwarted by the War of 1812.[80] After the peace, the public mind was more favorable to internal improvements; and Clinton, removed by Governor Tompkins in November, 1815, from the appointive office of Mayor of New York City, had leisure to push the project. On December 31, 1815, friends of the canal met at City Hall in New York and appointed a four-man committee to prepare a memorial to the Legislature.[81] Van der Kemp expressed his pleasure in seeing a notice of this meeting. "So my wishes have been anticipated—" he told Clinton. "God prosper it—be it through your influence and unrelenting activity! and posterity shall bless the name of De Witt Clinton." On the question of federal vs. state support of the canal, Van der Kemp was far more decided than Clinton that it must be a state project. "No New-Yorker," he averred, "ought to wish to obtain it from the federal government—it would claim the benefit—and exact the profit."[82] On December 2[illegible]16, Van der Kemp even went so far as the trea[illegible]le suggestion that the canal might become th[illegible]s of making New York "a powerful independen[illegible]vernment—perhaps in bonds of union and friendship with the Canadas."[83]

Clinton wrote to Van der Kemp on February 21, 1816, promising to urge the canal, and to send the New York Memorial recommending it. This document, presented to the state Legislature March 1, 1816, was Clinton's work—a masterly presentation of the method of construction, plan of finance, and commercial advantage of the canal. Sectional and personal hostilities at once came into play; Governor Tompkins refused to advocate any work that would glorify Clinton; Martin Van Buren killed the part of the bill authorizing immediate construction; and the counties along the lower Hudson and on Long Island opposed the whole project, fearing that it would cut down on their trade.[84] But a new canal commission was created, with Clinton at the head, Van Rensselaer carried over from the old commission, Joseph Ellicott, agent of the Holland Land Company, replacing Gouverneur Morris, and Myron Holley of Ontario County appointed treasurer. On April 14, when Van Buren raised his voice for the bill in the state Senate, the battle was won, and the grateful Clinton rushed across the floor to shake his enemy's hand. The plan of finance, by auction duties, tax on salt, and taxation of lands extending

to send to General Alexander Macomb, Chief of the Corps of Engineers, the diagram of an improvement for firing cannon, devised by Van der Kemp's friend Parker. On October 6, 1824, Clinton reported General Macomb's negative judgment of the invention.

[79] On his first exploratory trip westward, Clinton noticed the balance of power in nature in the fact that boneset, a remedy for swamp fever, grew profusely in the fever-generating country along the Seneca River and Wood Creek (Private Canal Journal—1810, Campbell, *The life and writings of De Witt Clinton*, 97).

[80] See V. d. K. to C., Sept. 14, 1812, note 36.

[81] These were Thomas Eddy, Cadwallader Colden, the only Tammanyite favoring the canal, and grandson of the Colden who in 1724 made the first survey of internal navigation, John Swartwout and De Witt Clinton. (The duel between Swartwout and Clinton in July, 1802, occasioned by the political rivalry between Clinton and Swartwout's friend Aaron Burr, was long since forgotten.

[82] V. d. K. to C., Jan. 8, 1816.

[83] Doubtless there were occasions during the War of 1812 (as when Clinton had to fortify New York harbor by means of municipal and private funds, lacking state or federal help) when Clinton might have shared Van der Kemp's desire that New York secede from the Union—but he never put such a thought in writing. Not until 1817, when President Madison vetoed the bill to divide among the states dividends in the bank of the United States, did Clinton give up hope of federal support and harden his determination to make the "Grand Canal" a New York State affair.

[84] Van der Kemp termed these oppositions "pusillanimous" (V. d. K. to C., Oct. 22, 1816).

for twenty-five miles on both sides of the Canal,[85] later underwent revision; but from the start it had the advantage of creating a fund, separate from the rest of state money, and of opening subscriptions to interested individuals or banking companies.

Van der Kemp had his own ideas about financing the undertaking. He suggested to Clinton on October 2, 1816, that the burden of taxation not be laid, as planned, chiefly on the landholders of the adjacent districts. A loan could easily be obtained, he thought, at London or Amsterdam, on the credit of the state or the city of New York.[86] Doubtless Clinton's plan was better fitted to the exigencies of the New York situation than Van der Kemp's proposal of a foreign loan. But one of Van der Kemp's suggestions, that the commissioners be paid (they were allowed expenses only, for tours of exploration and inspection) was shrewd[87]—and probably rejected by Clinton only because he realized that the Legislature would never allow this added expense. This plan for salaries would have saved the treasurer, Myron Holley, from some later embarrassment.

Although no engineer, Van der Kemp knew the terrain of central New York very well indeed, and his comments on the Canal *Reports* which Clinton sent him were detailed and penetrating. "You sent me Reports, Maps &c. documents," he wrote to Clinton on April 25, 1817, "two Reports"[88]—two Acts—with the *Treatise on Inland Navigation* and Gallatin's Report, but not one single map." On page 35 of the Commissioners' first report he noted the omission of the number of yards. On page 41, he quoted: "except a short distance of a cedar swamp *with a marsh bottom* but in fact hard and good ground!" "It had been worth to prove it," Van der Kemp objected. "How is it that hard and good ground was known? by the marsh! This sometimes covers quick-sand."[89] On page 62 he noted the Commissioners' objection to the allowance of 5 per cent for contingencies, and $1,000 per mile for engineers, superintendents, &c. Van der Kemp reminded Clinton that other expenses might well have been underrated, and a full allowance should be made, to cover errors.[90] For example, he objected to the adverb "probably" in the statement that there were rocks below the surface; and he scoffed at the adjective "little" for Antony's Nose—solid granite.[91] He was, however, delighted at the project of a short lateral canal to Salina. If by sinking a shaft of fifty feet the real salt bed might be discovered, it would be an inexhaustible treasure.

However idealistic Van der Kemp's vision of what the canal could do for the whole state, he was not free from the sectional feeling (every county for itself) which sometimes held up the progress of construction. "Is a lateral canal from Lake Ontario between Salmon River and Bunce's creek at Oneyda Lake," he asked Clinton on December 29, 1821, "contemplated before, and then

[85] Van der Kemp suggested confining the taxation to 12½ miles on both sides of the route, and taxing New York, Albany, Troy, Lansingburgh, Waterford, "and other large villages" in proportion to the amount they would gain from increased commerce (V. d. K. to C., April 25, 1817).

[86] Van der Kemp repeated this point on April 25, 1817, suggesting his banker friends David Parish of London and Paul Busti of Philadelphia (with connections in Amsterdam) as agents.

[87] "It might answer a good purpose—if in the detailed report was proposed the appointment of a committee of four, one from each district, with a competent salary—to superintend the construction of the canal—*till* it was finished. This might be the means to stop the mouth of some cerberus—never minding—to what party he belonged—provided he was an influential dog" (V. d. K. to C., Dec. 27, 1816).

[88] *Report of the Canal Commissioners*, De Witt Clinton, Stephen Van Rensselaer, Samuel Young, Myron Holley (Albany, Feb. 15, 1817); *The official reports of the Canal Commissioners of the State of New-York, and the acts of the Legislatures respecting navigable communications between the great western and northern lakes and the Atlantic Ocean: with perspicuous maps and profiles.* Published at the request of the Board of Canal Commissioners, New York, August, 1817. Van der Kemp's references here are all to the Feb. 15, 1817 Report, but he evidently had separate copies of the Reports and Acts, which were bound together in August.

[89] Here Van der Kemp referred to Mile 22nd (west of Rome, in the central section of the canal) 2 miles west of Canastota Creek. Actually this land did not give so much trouble as that of the Montezuma and Seneca marshes, where swamp fever caused severe illness and some loss of life among the workmen (Clinton to the New York Legislature, Jan. 2, 1822).

[90] Van der Kemp's advice was sound on this point; but, considering the pioneer nature of the project, the original cost estimate of $4,881,738 was admirably close to the actual cost of five million (Clinton to the New York Legislature, Jan. 3, 1826). Clinton was able to congratulate himself and the legislators at this time also on the total cost of both the Western (Erie) and the Northern (Champlain) canals, as $7,737,770, very near the original estimate.

[91] *Report* (Feb. 15, 1817), 73. This reference was to Mile 34 (eastern section) two miles west of Little Falls. Actually it was the Little Falls excavation that the New York engineers took most pride in. The English engineer Weston had estimated three years to do the job, which was accomplished in three months.

considered as practicable, entirely obliterated? I considered it so, and beneficent beyond calculation. If I possess[ed] as much wealth as good will, I would make a canal—entirely achieved—from this village—along the West Canada creek to the Mohawk." Van der Kemp reminded Clinton that there were sufficient small streams and lakes between Lakes Ontario and Oneida to act as feeders for such a canal.[92]

Van der Kemp showed some awareness of the political pressures on Clinton, in his letter of December 27, 1816. He warned his friend that in going to Albany from New York he would be passing into a cold climate. "Why not," he suggested, "arm yourself in time with a Spencer[93] by which a northern blast might be baffled—and eastern lukewarmness heightened to a blaze? . . . I know what powerful engines can be set at work by malice and self-interest."

In 1820 Clinton was re-elected Governor over Daniel Tompkins, but with a bare majority, saved only by the vote of upstate farmers who were beginning to prosper since the completion of the canal's central section. He saw trouble ahead with the appointment to the Canal Commission of two ardent Van Burenites, Henry Seymour and William C. Bouck. He concentrated all his knowledge of the state and his zeal for the Canal into his Hibernicus *Letters*, originally published as a series in *The New York Statesman*,[94] widely circulated and eminently successful. The constitutional convention of 1821 curtailed the governor's power of appointment, but it made the Canal Law inalienable by any future act of the Legislature, and firmly established the Canal Fund.

On April 20, 1822, Clinton wrote to Van der Kemp, promising to visit him in June on his return from the westward journey of canal inspection.

> When the *opus basilicum* is finished, I shall consider the State as in a situation to be as prosperous as she pleases; but wealth and prosperity, my friend, are too often the parents of folly, and the more opulent the State, the greater the temptation to the enterprises of parties.[95]

Stung by the increasing hostility of the Legislature, Clinton had already decided not to renew his campaign for governor. Van der Kemp cheered him with his letter of May 3, 1822, congratulating him on the conduct of the Albany meeting concerning the canal, and praising the speeches of General Gansevoort and Colonel Haynes.

On the trip of inspection in June, 1822, Clinton enjoyed watching the thick traffic in the completed central section. With his fourth son, De Witt, a tyro engineer, at his side, Clinton proudly surveyed the ascending and descending vessels, among them the *Lady Clinton*, which honored his second wife, Catherine Jones. Many problems still remained to be solved, the most pressing the question of where the canal should terminate, Black Rock or Buffalo. In the Commissioners' meeting at Buffalo, Clinton lost the first round of this battle to Peter B. Porter of Black Rock, Clinton alone of the group voting for Buffalo.

The political troubles besetting the Canal Commissioners were commiserated by Van der Kemp in his letter of March 20, 1823. Thanking Clinton for his notice of Jonas Platt's share in the original proposal of the canal, Van der Kemp cheered his friend with a favorite quotation: "*Mea me virtute involvo.*" "I pity our poor Political Jugglers," he wrote "and abhor their principles and curse their proceedings. No free government can exist long on such a rotten foundation."[96] What Van der Kemp failed to realize was that Clinton himself knew how to juggle the political balls, and that even now, temporarily out of favor in New York, he was mending political fences by planning visits on canal business to Ohio and New Jersey, with an

[92] This canal was recommended in the *Report* (Feb. 15, 1817) but a bill for it was not passed until 1832; navigation began on Sept. 12, 1835, on what was called the Oneida Lake Canal (Whitford, Noble E., *History of the canal system of the state of New York . . . ,* 2 v., 1: 656–657, Albany, 1906). Thus Van der Kemp, who died Sept. 7, 1829, did not even see the beginning of his favorite project.

[93] The pun needs some explanation. A spencer was a short jacket worn under a coat for extra protection in winter. Van der Kemp was also referring to Judge Ambrose Spencer, Clinton's brother-in-law, who for several years had sided with Van Buren against Clinton. Actually, Clinton had already anticipated this advice. Spencer, reconciled with Clinton, won his demand for a nominating convention, and Clinton was again elected governor, taking office July 1, 1817. John C. Spencer, Ambrose's son, was not always sympathetic with Clinton's policy, but proved a valuable spy for him on central New York affairs. John wrote to Clinton from Canandaigua, October 21, 1824, telling him of reports circulated against him by Commissioner Stephen Bouck, and warning Clinton that the contractors whom Clinton had been criticizing were listening to Bouck; furthermore, the contractors controlled a good many votes.

[94] See note 1.

[95] Seymour, *Centennial address*, 18.

[96] *Cf.* V. d. K. to C., April 21, 1823.

eye on the presidency. Van der Kemp further bolstered Clinton's courage in his letter of May 5, 1823:

> I fostered no doubt, that the canal would have been accomplished—but now I rejoice—and—perhaps—I may yet see the day—when at the junction of it with the North River a splendid monument shall be erected in the names of the men, so much deserving the thanks of posterity.

Here Van der Kemp touched a responsive chord in Clinton's heart. He was deeply disappointed when the canal was not named for him; and it is a curious irony that instead of the canal, the first New York Railroad train, which made a trip from Albany to Schenectady September 24, 1831, should have been named the De Witt Clinton.[97]

On April 8, 1824, Clinton wrote to Van der Kemp of his surprise in finding that this friend was one of the many claimants to the honor of "originating" the canal:[98]

> Your letter to Col. Mappa, on the canal, written in 1792, is really a curiosity. It gives you the original invention of the Erie route, and I shall lay it by as a subject of momentous reference on some future occasion.[99]

In September, 1823, with Chancellor Kent, Clinton watched the first passage of sloops through the lock and dam at Troy. Of the attendant festivities, he wrote to Van der Kemp from Albany on October 11:

> The canal celebration was splendid and felicitating. For thirteen years I have labored to produce this magnificent result. I have encountered opposition in all its forms from honest apprehension down to confirmed knavery—Almost all the leaders of party have at one time or another been arrayed against the measure—And nothing but the special favor of providence could have enabled its friends to surmount the difficulties that were presented at every step. For the abuse that I have received I have no resentments to gratify. Success has extinguished all unkind feeling against unjust opposition and I cordially cherish that catholic spirit which forgives causeless enmity and malignant calumny.

Clinton's glow of satisfaction was genuine, but premature. On April 12, 1824, the last day of the legislative session, a Tammany plot was brought to a carefully planned climax. In the closing hours, John Bowman proposed a resolution to the Senate that Clinton be removed from the office of Canal Commissioner. The resolution, actually the work of the Lieutenant Governor, Erastus Root (whom Clinton called "the root of all evil"), and John Suydam of Kingston, an ardent Federalist and long-time foe of Clinton, was pushed through the Senate and rushed to the Assembly, which approved it by a vote of 64–34, and Erastus Root exulted that his brilliant *coup* had killed De Witt Clinton as a political figure.

Behind the maneuver was a tangle of motives—Porter's revenge for the Black Rock affair, Holley's difficulties, Elkanah Watson's retaliation for Clinton's neglect, and the long slow growth of political and personal hostility. But the truly sinister purpose did not appear. Despite the label of "aristocratic" which stuck to Clinton because of his friendships with Livingstons, Van Rensselaers, and Van Wycks, he had a sincere belief in popular sovereignty. For years he had advocated a change in the state electoral law, to nominate president and vice president by the people directly rather than by legislative or convention caucus. During the August session of the Legislature, called by Governor Yates to effect a change in the law, the move was defeated, chiefly because Van Buren's supporters did not dare trust a popular method of nomination. With the time for the caucus approaching, to discredit Clinton by ejecting him from the only honorary office he held not only put the final stamp of approval on the caucus method—it also disqualified Clinton for the nomination. Terrified of political ostracism, Clinton's friends did not dare protect their former leader even from this obvious injustice.

The next evening, April 13, the Republican

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[97] His Diary (New-York Historical Society) contains an interesting section on railroads vs. canals, in which he does justice to the railroads; but his service to them was small, compared to his far-reaching and long sustained work for canals. The Society's permission to cite this is gratefully acknowledged.

[98] Among the claimants to this honor (a dubious one, in the early days of construction), Clinton did justice to his friend Gouverneur Morris, but particularly praised Jesse Hawley, not only for the idea, but for the plan he laid out in *Observations on canals*, written from a debtors' prison in Canandaigua and printed in *The Geneva Messenger* in 1807. Clinton read Hawley's essays diligently on his first westward trip in 1810. Elkanah Watson, founder of the State Bank of Albany, was the fiercest champion of himself, bitterly reviling Clinton in the Albany newspapers for his refusal to recognize Watson's claim.

[99] This was Van der Kemp's statement: "Go on then and dig canals through the western district, and be not afraid that a single hair shall be hurt on the head of its inhabitants by the waves of Lake Erie" (*Letters from . . . Van der Kemp to . . . Mappa*, Appendix, Seymour, *Centennial address*, 56). Since this work of Van der Kemp existed only in manuscript at this time, it would have been hard to make a strong case for Van der Kemp as the originator of the Erie route.

caucus met in Albany and nominated Andrew Jackson for president.

As soon as Van der Kemp saw the Albany papers, on April 19, he sent Clinton a warm letter of sympathy. Indignant at the "foul misdeed," he yet turned his fury into a note of congratulation: "These fellows cannot bear the lustre of your deeds—it humbles them in the dust—I rather should prefer the most outrageous removal above a vote of thanks from a similar gang. They dare not lift up their eyes in your presence." Van der Kemp summed up his views: "A glorious exit—equal in renown to that of Aristides."

Van der Kemp somewhat overstated the case when he said that Clinton's enemies were digging their own graves—but in fact, no political gun ever backfired with a louder bang. The fatal flaw in the Tammany plot was that Clinton's opponents could bring no real charges against him. He had served the Commission without salary, and he had resisted the temptation that trapped Myron Holley, to buy lands along the line of the canal or to make investments in any way connected with the job. On April 16, 1824, the patriarch John Taylor, an old opponent of Clinton, addressed a protest meeting of Albany citizens at the capitol. This was followed by a meeting in New York City on the nineteenth, with another former enemy, Col. William Few, in the chair; and by similar mass meetings in the next few weeks at Rochester, Geneva, and Buffalo. Protests appeared in the newspapers of Ohio, Virginia, and Pennsylvania. In September, Clinton was the popular nominee for governor, and in November he won the election by a landslide from Samuel Young, who was supported by Tammany and the People's Party. The cup was further sweetened by the victory of James Tallmadge over Erastus Root, for Lieutenant Governor.

In the fall of 1825 New York State girded her loins for the biggest celebration in history—the completion of the Grand Canal. Art, music, oratory, drama, fireworks—every conceivable device was brought into play to dramatize the "Meeting of the Waters." Committees taxed their wits for ways to commemorate the union of commerce and agriculture, the successful achievement of an American republican government, and the glory of the Empire State. All agreed that the theme of rejoicing was too sublime to admit of exaggeration—this was the time to let the metaphors blaze and the flags fly.

The festivities began at Buffalo on October 26; from this point the flotilla of boats, headed by *The Seneca Chief* in which the Governor and Mrs. Clinton were passengers, made the triumphal progress through the canal, pausing for congratulatory dinners at all the principal cities along the route. The celebration ended in New York City on November 7 with a Grand Canal Ball. Although Olden Barneveld was not on the route of the procession, Van der Kemp delivered a ten-page address to his fellow citizens on October 26, 1825, going back over his favorite ground of the American fight for independence, hailing the canal as a further advance, through its assistance to agriculture and trade, praising the work of the Canal Commissioners, with special emphasis on Clinton's fearless leadership—and mentioning his own early idea concerning the Erie route.[100] A year later, on the anniversary of the opening, he sent Clinton a briefer, two-page tribute, in a more poetic vein, concentrating his praise on Clinton. Using Neptune as his central figure (here he was in the main stream of contemporaneous paeans), he sounded the international note—not, as one might expect, by reference to the canals of Holland, but to the first European canal, that of Languedoc, constructed during the reign of Louis XIV. He had the chorus of "Clinton—Clinton" sung by Tritons and Nereids, reechoing from the European, Asiatic, and African coasts. He concluded with the Italian proverb which he had often quoted to cheer Clinton's darker hours: "Tutto si puo, quando si vuole."[101]

If building the canal was Clinton's chief service to New York State, the translation of the Dutch records of New Amsterdam (known as the Albany Records) was Van der Kemp's. Their cooperation on this project is another story, mentioned here as a further link in the strong chain of a remarkably productive friendship. Although less interested than Clinton in such social issues as public education,[102] prison reform, slavery, and the Indian question, Van der Kemp nevertheless made comments on all these matters. Some pamphlets on slavery, sent by Clinton, aroused Van der Kemp's Republican sentiments:

[100] MS., New-York Historical Society.

[101] Van der Kemp would have liked to publish this, but his friends Tyng and Platt, appreciative though they were of its sentiment, advised against publication. "It was no wreath for Clinton," Van der Kemp wrote regretfully.

[102] V. d. K. to C., July 28, 1824, praises a speech by Clinton to the Education Society, but criticizes his use of the word "feeble" applied to his own efforts.

I always abhorred slavery—in what shape it might appear—but suspected never that free enlightened Englishmen could be guilty of such a heinous crime. . . . Would to God that such [*sic*] foul a blot could be wiped off from the American name—It must cause finally the ruin of the Southern States—if not soon some heroic medicine is applied.

Van der Kemp's "medicine" was to start trial plantations for some superior slaves, at a small rental, looking ahead to complete emancipation.[103] The more pressing problem in New York of Indian rights and the adjustment of their claims, which occupied Clinton's attention as governor, interested Van der Kemp less than Elias Boudinot's speculation that the American Indians were descendants of a "lost tribe" of Israel.[104]

Along with the serious themes which weave in and out of the correspondence, there are lighter moments. Frequently Van der Kemp expatiates on the charm of the West Canada Creek Falls, as a lure to Clinton's "Lady" to visit Olden Barneveld with her husband.[105] At Clinton's request, Van der Kemp sent an account of the Flemish *émigré* Des Wattines, whose wild American adventures became a part of the period's folklore. Having stolen a respectable girl from a convent, Des Wattines fled to Virginia, where he was robbed of most of his fortune. In New York he was again cheated by a Frenchman, M. Bernet, who lured him to Oneida Castle on the pretense of starting a colony, but abandoned him to the wayward mercy of the Indians. George Scriba gave him a farm, which he neglected; eventually he reached New York City, traveling by canoe. Scriba raised the money to send him back to France, but he complained that Scriba did not give him enough to travel in a manner suitable to a gentleman. Van der Kemp told of Des Wattines' drowning a large bear by slipping the rope of a canoe around his neck, while his daughter was in the canoe. Van der Kemp's personal relations with him involved Des Wattines' request for the bodies of two of Van der Kemp's oxen, drowned in Oneida Lake, out of which the immigrant made what he called *une soupe excellente;* and Van der Kemp's reproof of Des Wattines when his dogs devoured two of the Dutchman's prize ewes, given him by Chancellor Livingston. Des Wattines could not believe that his dogs had done this deed: "Parce que mes chiens ont reçu leur éducation en France."[106]

A break in the notable friendship between Clinton and Van der Kemp occurred in 1824—caused, not surprisingly, by misunderstanding over a book. Van der Kemp had sent a quarto Greek *Testament* to Clinton, requesting that he deliver it to the Rev. Mr. John De Witt. Hearing nothing about it from either Clinton or De Witt, the deeply injured Van der Kemp wrote to Jonas Platt, October 20, 1824, asking him to sound out Clinton on the matter. "I can easily pardon the forgetfulness of such great folks," he told Platt, "in not caring much about a poor recluse of Olden Barneveld." Platt forwarded the letter to Clinton. Clinton hastened to make amends, and the friendship was resumed.[107] Since Van der Kemp's eyesight was not equal to the type in the Albany *Gazette*, Adam Mappa read aloud Clinton's message at the opening of the Legislature on his triumphal return in 1825, and Van der Kemp promised a fuller comment when he had had a chance to study the message more carefully.[108]

Van der Kemp's letter of February 7, 1827, reported the "masterly" operation for cataract on his left eye by his friend Dr. Luther Guiteau, which was followed by excruciating pain, and no recovery of sight, a disappointment which Van der Kemp was bearing with stoical fortitude.

On January 29, 1828, the seventy-five-year-old Van der Kemp wrote to acknowledge Clinton's letter of January eighth, and to forgive his friend's silence since the spring of 1827. Of Clinton's political career, Van der Kemp wrote: "Whatever the final result may be, Clinton, I am confident, shall remain in possession of the respect and admiration of our State and of a great part of the Union." Congratulating Clinton on a toast delivered at an Albany celebration, he said: "Mine would have been, had duty compelled me to assist, 'Our next Republican President—and the Father of the Erie Canal. . . .'"

In a despondent moment over the death of dear friends, especially that of Gerrit Boom, Van der Kemp had written, October 25, 1826: "Alas! my

[103] V. d. K. to C., Nov. 3, 1824. Clinton's continued efforts to abolish slavery were at last successful, but under the administration of his successor, Daniel Tompkins, July 4, 1827.

[104] *Cf.* note 41.

[105] See, e.g., V. d. K. to C., May 3, 1822.

[106] V. d. K. to C., March 1, 1816. *Cf.* note 23.

[107] Van der Kemp's letter to Platt is in the Clinton Papers, as is V. d. K. to C., Nov. 3, 1824, referring to C. to V. d. K., Oct. 11, 1824 (no copy in Clinton Papers).

[108] V. d. K. to C., Jan. 11, 1825. Even one reading of the address prompted Van der Kemp to question Clinton's use of the word "talented," which Clinton applied to the members of the United States Senate. Furthermore, he urged Clinton to hasten the settlement of his account with Bayard's firm in New York City.

dear Clinton! What is human happiness? . . . Ere long I must remain alone—but, thanks to a bountiful God, I am yet remembered by a few—and among these, I trust you will remain to the last."

Van der Kemp's trust was literally fulfilled. On February 6, 1828, he wrote to Clinton concerning a political appointment he sought for a friend, and a book he had sent to James Renwick.[109] Clinton spoke to the Senate on February 11, saw visitors, and wrote several letters—among them, the final one to Van der Kemp, promising as usual to carry out Van der Kemp's requests. By evening, Clinton was dead.

> My worthy friend [Clinton wrote], Your letter of the 6th has been received. It required no apology. You have a right to speak to me at all times, on all occasions, and on all subjects. . . . I am Dr Sir Now and always Your sincere friend De Witt Clinton."

This is the final grace note in a correspondence which rings many changes on the interaction between two brilliant men: Van der Kemp more scholarly, philosophical, retrospective; Clinton more active, dynamic, practical; throughout their letters resounds the common ideal of identity: devotion to the advancement of learning and technocracy in the American republic.

[109] Not in Clinton Papers, but referred to in C. to V. d. K., Albany, Feb. 11, 1818, Buffalo Historical Society. The Society's permission to quote is gratefully acknowledged.

THE UNDERGROUND RAILROAD: LEGEND OR REALITY?[1]

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(*Read November 11, 1960*)

FEW stories in our national history are more familiar to Americans than those of the underground railroad with its legendary heroes. Generations of youngsters have thrilled to the account of Eliza Harris eluding the slave-catchers and their bloodhounds by leaping from ice cake to ice cake on the thawing Ohio River, and to her later adventures as a passenger on the underground line. Along with Paul Bunyan and Mike Fink, Eliza and some of the underground railroad conductors have become figures of American folklore. Much of the underground railroad story has been handed down by oral tradition with its inevitable mixture of fact, fancy, and exaggeration. Writers of popular accounts and local histories, journalists, antiquarians and scholars have all been influenced by the legend and have repeated some of its unproved assertions in their writings.

Like most legends, that of the underground railroad is vague and indistinct. However, in the minds of many people there is an image of the mysterious institution with which they associate certain general characteristics. One of these is the existence of a highly organized network of underground railroad lines, with stations literally dotting the North and penetrating into the South. In the popular mind, at least, a board of directors managed the road's affairs and gave it the benefit of centralized control.[2] The author of a semi-fictional account claimed that for more than twenty years the underground railroad "extended its great trunk lines across all the northern states from Mason and Dixon's line and the Ohio River to the Queen's Dominion, and its ramifications far into the southern states."[3]

Another characteristic of the legendary underground railroad was secrecy. According to a 1936 newspaper account, it was "the most successful secret organization that ever existed in this country."[4] In the legend most of the fugitive slaves, like Eliza, are only a jump or two ahead of their would-be captors, but the adroit conductors outwit the slave-catchers through the use of secret code signals and numerous hiding places. They had, said a popular book on the subject, "false closets through trap doors in kitchens or parlors, false cupboards over brick ovens, sliding panels by fireplaces where wood was stowed, secret rooms without windows."[5] There is also a persistent belief that the need for secrecy led to the destruction of all the records and documents concerning the work of the underground. Publicly, "there was no Underground Railroad," said a student of the Pennsylvania branch. "Had the existence of it even been suspected, the Government or the kidnappers would have wiped it out."[6]

In the context of the popular legend the underground railroad was a very busy enterprise, running its numerous trains nearly every night. There is a distinct impression of an extremely heavy traffic on the underground line. A Pennsylvania newspaper story of underground railroad activity in the state commented, "Thousands of slaves

[1] The author is indebted to the Research Committee of the American Philosophical Society for a grant from its Penrose Fund which made possible an extensive research trip to gather material for this study. This article partially summarizes the findings presented in a book-length study entitled *The liberty line: the legend of the underground railroad* to be published by the University of Kentucky Press.

[2] For an excellent example of the use of this concept in fiction, see Stern, Philip Van Doren, *The drums of morning*, 325–432, New York, Doubleday, 1942.

[3] Johnson, Homer U., *From Dixie to Canada: romances and realities of the underground railroad*, 12–13, Orwell, Ohio, H. U. Johnson, and Buffalo, C. W. Moulton, 1894.

[4] Clipping from the *Cincinnati Enquirer*, March 8, 1936, in the Wilbur H. Siebert Papers in the Ohio Historical Society. (All Siebert Papers hereafter cited are in the Ohio Historical Society.)

[5] Buckmaster, Henrietta, *Let my people go: the story of the underground railroad and the growth of the abolition movement*, 199, Boston, Beacon Press, 1959.

[6] William Hutchinson Smith to Wilbur H. Siebert, December 28, 1933, in the Siebert Papers.

passed through these stations for thirty years." [7] When a writer recently sent out a questionnaire requesting several hundred people to guess at the number of slaves who ran away from the South between 1851 and 1860, the average answer was two hundred and seventy thousand, or twenty-seven thousand each year. Several answered a million.[8]

In the romanticized legend it is the abolitionist, or underground railroad conductor, who stands in the spotlight. He is the hero. The operators of the underground railroad, as described in a recent book, lived in a time that called for greatness, "and they had the greatness within them." [9] They were willing to risk all for the cause they served. The stereotype of the heroic abolitionist is matched by that of the ignorant and helpless fugitive. Although the fugitives of the legend are determined to be free, they lack the necessary knowledge and skill to reach the Promised Land without the assistance of the underground transportation system.

The legend of the underground railroad is a combination of fact and fancy. Many of the stories handed down by word of mouth had a factual basis, but frequent repetition has led to exaggeration and sometimes, in the annals of local history, fantasy has become fact. Far too much of the underground railroad's history rests upon the reminiscences of aged abolitionists written many years after the events had transpired, and after their cause had become respectable. The underground railroad of historical reality existed, but it was markedly different from the institution of the popular legend.

There was no nationwide conspiracy to spirit fugitive slaves to Canada. It was largely a matter of sheltering and assisting fugitives who had already escaped. Abolitionists held different opinions concerning the importance of underground railroad activity. Only a very few of them approved of enticing slaves from their masters or taking them from the southern states. Some thought that such activity was unethical and most believed that it was unwise. Even the extremist Oberlin anti-slavery reformers refused to sanction the program of three of their group who ventured into the South to guide slaves to freedom. Members of the Philadelphia Vigilance Committee also disapproved of sending agents south to assist slaves to run away, although in a few instances they did abet such escapes from southern bondage.[10]

When abolitionists provided assistance to the absconding slaves in the form of food and other necessities, temporary lodging, and transportation, it was often on a haphazard basis. Some abolitionists who took a special interest in this phase of anti-slavery activity organized underground railroad work in certain local areas, but at no time did such efforts become centralized. Levi Coffin, for example, introduced a semblance of order into the underground railroad efforts in the parts of Indiana and Ohio where he lived.[11] Similarly, Thomas Garrett took the initiative in the Wilmington-Philadelphia area, where he had the active cooperation of a group of militant anti-slavery Quakers in Delaware and neighboring counties of Pennsylvania and of the Philadelphia Vigilance Committee.[12]

The vigilance committees, organized in a number of northern cities, assumed special responsibility for giving assistance to the bondsmen fleeing from slavery. Those committees which were founded before the passage of the Fugitive Slave Law of 1850 were sometimes brought into being by free Negroes who, in certain communities like New York City, worked wholly on their own, without ties to any abolitionist society. The New York committee reported in 1837 that it had protected 335 persons from slavery. David Ruggles was the competent and energetic Negro secretary of the New York committee. In 1845 a group of New England Negroes formed a Freedom Association with the object of extending "a helping hand to all who may bid adieu to whips and chains, and by the welcome light of the North Star, reach a haven where they can be protected from the grasp of the manstealer." [13]

After 1850 the abolitionists created a number of

[7] Clipping from the *Upper Darby News*, August 5, 1954, in the Chester County (Pa.) Historical Society.

[8] Furnas, J. C., *Goodbye to Uncle Tom*, 239, New York, William Sloane Associates, 1956.

[9] Breyfogle, William, *Make free: the story of the underground railroad*, 35, Philadelphia and New York, Lippincott, 1958.

[10] Report of an interview between Wilbur H. Siebert and James H. Fairchild, August 3, 1892 in the Siebert Papers; Still, William, *The underground rail road*, 177, Philadelphia, Porter and Coates, 1872.

[11] Coffin, Levi, *Reminiscences of Levi Coffin, the reputed president of the underground railroad. . . . passim*, Cincinnati, R. Clarke and Co., 1880 (hereafter cited as *Reminiscences*).

[12] Drake, Thomas E., Thomas Garrett, Quaker abolitionist, in *Friends in Wilmington, 1738–1938*, 75–86, Wilmington, Del., n.d.

[13] Aptheker, Herbert, ed., *A documentary history of the Negro people in the United States*, 162–163, 253–254, New York, Citadel Press, 1951.

new vigilance committees. Among the best known were those of Philadelphia, Syracuse and Boston. Their object was to give aid to the fugitives, to protect free Negroes from kidnappers and to obstruct the Fugitive Slave Law. Those committees would have been unnecessary had there been in operation a centralized network of underground railroad stations. The rescue of alleged fugitives from slave-catchers or from officers of the law provided exciting diversion for a number of the vigilance committees. One of the most famous rescues was that of the slave William Henry, or "Jerry," in Syracuse in 1851. Celebrating the anniversary of the Jerry Rescue became a regular event with the abolitionists and provided effective propaganda for the anti-slavery cause. An announcement of one such celebration said that the rescue had been "the heroism of the Right" which "stirred the hearts, and quickened the pulse, of the Friends of Freedom throughout America." [14]

Although most abolitionists used the fugitive issue for propaganda purposes not all of them took an active part in the underground railroad. Some even questioned the validity of such work. The major objective of the abolition movement was to end slavery rather than to assist a few fugitives to escape from it. Abolitionists made a distinction between the two efforts and they deplored any tendency of the lesser cause to divert energy and attention from the greater. Mrs. Sarah Otis Ernst of Cincinnati, for example, feared that the abolitionists' "energies and funds would be so frittered" away by such things as a colored orphan asylum and the running off of fugitives that they would have no means left for their "higher and more important work." [15] For the abolitionists the propaganda aspects of the fugitive issue were even more important than the fate of an individual fugitive. In 1850 a Chicago abolitionist newspaper suggested that it might be "expedient for some fugitives to suffer martyrdom . . . and consent to return awhile to slavery as propagandists of liberty, and as a standing appeal to the humanity of the North." [16]

Those abolitionists who emphasized working with the fugitive slaves often carried on their underground railroad activity with very little attempt at secrecy. Levi Coffin recalled that slave-catchers frequented Cincinnati but that none ever bothered him, even though his reputation as an underground railroad conductor was no secret to anyone in the area.[17] Thomas Garrett openly boasted of the large number of fugitive slaves he had helped to freedom.[18] Although anyone who refused to obey the Fugitive Slave Law was legally liable for punishment, the government seldom prosecuted violators. In 1853 an anti-slavery newspaper in Ohio commented, "not even one of the thousands who have refused obedience to this law, has yet been condemned." [19] Actually, there were probably fewer than a dozen prosecutions under the act during its fourteen-year existence, although in most of those cases there were a number of defendants.

Moderation also characterized the use of the law to return individuals to slavery. Only fifty alleged slaves had been arrested by the spring of 1853 when the Massachusetts Anti-Slavery Society commented in its annual report that the law "was but an electioneering trick, not designed nor expected to be of material advantage to the Slaveholders." [20] Not all the runaway slaves were pursued, and very few were pursued by their masters. The expense involved in tracking down a fugitive slave was often greater than the value of one known to be addicted to running away. A few dramatic incidents involving slaves who had been pursued have given a false impression of pursuit as a common occurrence.

The abolitionists sometimes advertised their underground railroad services in newspapers. As early as 1844 the Chicago *Western Citizen* published a cartoon captioned "Liberty Line" which pictured the underground train chugging its way to Canada with a full cargo of jubilant fugitive slaves. Under the drawing was a humorous description of the "improved and splendid Locomotives" and "best style" passenger accommodations for those "who may wish to improve their health or circumstances, by a northern tour." [21] This was only one example of a number of similar notices. In 1856 a Syracuse paper carried an item telling of eight fugitives who passed through on their way to Canada. The Reverend J. W. Loguen, a former slave, was a key person in the Syracuse Vigilance Committee. "Brother Loguen," said the article, "talks of keeping a Hotel

[14] New York *National Anti-Slavery Standard*, September 16, 1852.

[15] Mrs. Sarah Otis Ernst to Miss Weston, February 6, 1852, in the Weston Papers in the Boston Public Library.

[16] Chicago *Western Citizen*, November 5, 1850.

[17] Coffin, *Reminiscences*, 118.

[18] Boston *Liberator*, May 22, 1857.

[19] Salem, Ohio, *Anti-Slavery Bugle*, March 19, 1853.

[20] *Twenty-first annual report presented to the Massachusetts Anti-Slavery Society by its board of managers, January 26, 1853*, 43, Boston, 1853.

[21] Chicago *Western Citizen*, July 18, 1844.

Register, after the manner of aristocratic establishments, for the purpose of recording the names of his guests, and having them published." [22]

There was a great deal of underground railroad material in the anti-slavery press. Abolitionist editors reported all stories of slave escapes that came to their attention. On occasion they notified the slaves' owners that their chattels had crossed the border into Canada where they were safe from all fugitive slave laws. In March of 1856 the New York *National Anti-Slavery Standard* reprinted an advertisement from a Richmond paper offering rewards for the return of Henry and Tazewell. The *Standard's* editor commented that the owners could save themselves the expense of further advertisement. "Tazewell and Henry," he wrote, "both passed over 'tother side of Jordan,' one day last week, 'shouting happy.'" [23] In reporting such stories the reformer editors seldom distinguished between fugitives who had received assistance from the abolitionists and those who had accomplished their flight wholly on their own resources.

The publication of underground railroad stories was of no practical benefit to the fugitive slaves, and neither were the pronouncements of the anti-slavery politicians. The anti-slavery faction in Congress found the fugitive slave issue an especially useful one. The passage of the Fugitive Slave Law of 1850 brought forth a flood of speeches. Anti-slavery congressmen used the unpopular law to arouse pride in the North's free institutions, to criticize the South's violation of civil liberties, to challenge the power of southern leadership and to attempt to strike fear into the hearts of the slaveholders. They used strong language in condemning the legislation. "In the long catalogue of public crimes among civilized nations, there is none more cruel and barbarous than the fugitive slave law . . ." said Wisconsin's Representative Charles Durkee. Congressman A. P. Haskell of New York maintained that the law had "no parallel for its monstrosity since the time that the English wrested Magna-Charta from their monarch. . . ." [24]

In 1852 Congressman Joshua R. Giddings of Ohio assured his colleagues who talked of enforcing the Fugitive Slave Law that "*it cannot be done.*" [25] In 1860 New York's Congressman Charles B. Sedgwick said, "It must have been expected that so infamous a law would have been evaded by underground railroads, and by all other honorable methods." He denied that there was any difference of opinion on the issue in the North. "All parties wink at its evasion," he commented, "and all sympathy is with the fugitive." [26]

Repetition of such material helped to create a false impression concerning the number of passengers on the underground railroad. Complaints of southerners about extensive losses suffered by their constituents because of the abolitionist underground railroad strengthened the impression. Southern spokesmen tended to take the propaganda statements of the abolitionists and the anti-slavery politicians at face value. Extremist defenders of southern rights found underground railroad rumors valuable for their criticism of the North. Delegates to the Nashville Convention of 1850 listed among their grievances against the northern people the forming of organizations "to carry off slaves from the South," and protecting them from recapture.[27] Early in 1861 Tennessee's Governor Isham G. Harris charged that northern opponents of slavery had "run off slave property by means of the 'under-ground railroad,' amounting in value to millions of dollars." [28] Such statements could not be proved; like the propaganda barrage of the abolitionists, they were meant to sway men's minds, not to provide them with accurate information.

Many years later those abolitionists who wrote reminiscent accounts of their underground railroad service added immeasurably to the legend of the mysterious road. The aged crusaders wrote of the stirring times when they alone had opposed the minions of slavery. With the emancipation of the slaves their cause had triumphed and the abolitionists became the heroes of their own thrilling narratives.[29] One of them wrote, "Considering the

[22] New York *National Anti-Slavery Standard*, July 5, 1856, quoting the *Syracuse Journal*.

[23] New York *National Anti-Slavery Standard*, March 29, 1856.

[24] *Congressional Globe*, 32 Congress, 1 session, appendix, 886, August 6, 1852, and 585, May 17, 1852.

[25] *Congressional Globe*, 32 Congress, 1 session, appendix, 740, June 23, 1852.

[26] *Congressional Globe*, 36 Congress, 1 session, appendix, 179, March 26, 1860.

[27] Resolutions and address of the Nashville Convention, in the New York *National Anti-Slavery Standard*, June 27, 1850.

[28] *Message of His Excellency Isham G. Harris, to the General Assembly of Tennessee, in extra session, January 7th, 1861*, 8, Nashville, 1861.

[29] Autobiographical accounts of underground railroad conductors include: Coffin, *Reminiscences;* Fairbank, Calvin, *Rev. Calvin Fairbank during slavery times*, Chicago, Patriotic Publishing Co., 1890; Butler, Marvin Benjamin, *My story of the Civil War and the under-*

kind of labor performed, the expense incurred and the danger involved, one must be impressed with the unselfish devotion to principle of the men and women thus engaged."[30] Another said that he "had no apology to make for his book." He thought it right "that the young people should know how things were carried on during the fifties by the pro-slavery people who had control of the government."[31]

The reminiscers seldom included statistics but in retrospect their underground railroad became greatly magnified, both in terms of the number of slaves they had assisted and in terms of the importance of that service to the fugitive slaves. It was a matter of distance lending enchantment to the view, as one of them admitted.[32] Yet the underground railroad epoch, as seen from the perspective of the abolitionists, is basically the one which has found popular acceptance. It has been repeated in newspaper articles, local histories, works of fiction, and even in some reference works and scholarly monographs.

Usually there was a grain of truth in the underground railroad stories. They are exaggerated rather than wholly false accounts. Because of the romantic nature of the material it easily lent itself to exaggeration. In some localities, at certain times, the underground railroad was a flourishing institution. In 1856 J. Miller McKim of the Philadelphia Vigilance Committee wrote an Irish friend, "The 'Underground' is becoming a great institution. We had 25 arrivals last week, all within the space of 48 hours."[33] Such a dramatic event made a lasting impression on the abolitionists and those who read about their exploits. Yet, even for the active Philadelphia committee, it was an unusual amount of business. When William Still published his documentary account of his eight years of service with the committee he listed only about eight hundred fugitives who had used its facilities.[34]

Actual figures of underground railroad traffic are difficult to compile. Scholars have mistrusted the census statistics which indicate that about a thousand slaves a year escaped from the South, even though some contemporaries, including some of the abolitionists, accepted those figures as substantially correct. For example, Gamaliel Bailey, editor of the abolitionist newspaper *The National Era*, cited the census of 1850 as proof "that the number of fugitive slaves in the North has been greatly exaggerated."[35]

Certainly a very small percentage of the millions of slaves in the South escaped or attempted to escape to the North or to Canada. Temporary running away was much more common and should not be confused with escape from slavery. When slaves disappeared, those responsible for finding them seldom assumed that they had fled to the North. The runaways often returned voluntarily after hiding out for a time in the woods, in a nearby swamp or at a neighboring plantation. In truth, the great majority of slaves had no alternative but to accept their situation. They had practically no accurate information about other parts of the country. Furthermore, the status of free Negroes, both in the North and the South, was probably not attractive enough to inspire the average slave to undertake the highly dangerous plunge for freedom. The slaves who ran away permanently were unusual individuals, either constitutionally incapable of adjustment to the slave system or so sensitive and intelligent as to make the break regardless of the heavy odds against them.

The slaves who absconded from bondage were generally self-reliant individuals who planned and carried out their own escapes. Some literally walked away from slavery, traveling by night and hiding and resting by day. Many slaves knew about the north star and they also obtained directions from other slaves, free Negroes and sympathetic southern whites. One group of fugitives from Virginia traveled only at night for three weeks before they reached Pennsylvania.[36] A fugitive slave from Tennessee journeyed five

ground railroad, Huntington, Ind., United Brethren Publishing Establishment, 1914; Cockrum, William Monroe, *History of the underground railroad as it was conducted by the Anti-Slavery League*, Oakland City, Ind., Press of J. W. Cockrum Printing Co., 1915; and Pettit, Eber M., *Sketches in the history of the underground railroad . . .*, Fredonia, N. Y., W. McKinstry and Son, 1879.

[30] Butler, *My story of the Civil War and the underground railroad*, 180.

[31] Cockrum, *History of the underground railroad*, foreword v.

[32] Sloane, Rush R., The underground railroad of the firelands, *The Firelands Pioneer*, new ser. **5**: 35, 1888.

[33] J. Miller McKim to Richard D. Webb, April 4, 1856, in the Garrison Papers in the Boston Public Library.

[34] Still, *The underground rail road, passim.*

[35] Washington, *The National Era*, June 19, 1851.

[36] Smedley, Robert C., *History of the underground railroad in Chester and the neighboring counties of Pennsylvania*, 228, Lancaster, Pa., Office of the Journal, 1883.

weeks to reach Michigan.[37] Some made good their escape from the deep South, alone and unaided.

Slaves heading north often took advantage of available water transportation. Steamships running from southern ports sometimes carried fugitive slaves, either as stowaways or as passengers hidden with the consent of the ship captain or crew members. Despite the risks involved, some ship captains made extra money carrying such cargo. They were not necessarily abolitionists, but were willing to carry whatever kind of freight would bring them the best profit.[38]

Some of the escaping slaves devised ingenious methods to implement their desire to reach free soil. Mulattoes often passed as white travelers. Fugitives sometimes posed as free Negroes, carrying forged free papers to prove their status should it become necessary. The famous ex-slave Frederick Douglass used a sailor's free papers in his flight from Maryland. Disguises were common. Men posed as women, and women as men. When William and Ellen Craft escaped from Georgia, Ellen, who was nearly white, posed as an ailing planter and William as his trusted servant. They traveled by train and had little difficulty along the way.[39] Henry "Box" Brown was only the most famous of a number of individuals who escaped or attempted to escape by having themselves crated and shipped to the North.[40]

When the fugitives did somehow find themselves on the underground railroad it was usually only after they had already completed the most difficult and dangerous stretch of their journey without such assistance. One fugitive from Virginia located the underground railroad in Ohio after getting that far by himself.[41] Another, from Nashville, Tennessee, reached Ottawa, Illinois before contacting an abolitionist who helped him get to Chicago.[42] Still another arrived in New York after a three-week ordeal of "sleeping in the woods and caves by day and traveling at night."[43] The assistance given these fugitive slaves by the abolitionists was helpful but not necessarily essential for the success of their flight from slavery.

The fugitives themselves deserve more recognition than they have received in the popular legend of the underground railroad with its emphasis on the abolitionists and their role in the exciting drama. The legend has contributed to the popular idea that large numbers of slaves traveled to freedom on the underground line, and it has presented later generations with a distorted view of the workings of the underground railroad itself. Based partly on the writings of aged participants, the legend of the underground railroad has been further entrenched in the popular mind by the addition of countless local folk tales and unproved traditions. Elements of it have been repeated by some scholars as well as popular writers and journalists. Yet the over-simplified picture of the past as presented in legendary accounts should not be accepted as history. The actual history of the institution is much more complex and even more interesting than the legend itself.

[37] Haviland, Mrs. Laura Smith, *A woman's life-work* 213, Chicago, 1889.

[38] Still, *The underground rail road*, 74–75, 166, 263.

[39] Still, *The underground rail road*, 368–377; *Running a thousand miles for freedom; or the escape of William and Ellen Craft from slavery*, London, 1860.

[40] Stearns, Charles, *Narrative of Henry Box Brown*, . . . , 59–62, Boston, 1849.

[41] New York *National Anti-Slavery Standard*, February 3, 1853, quoting the New York *Tribune*.

[42] Drew, Benjamin, *The refugee: or, the narratives of fugitive slaves in Canada*, 314–320, Boston, J. P. Jewett; New York, Sheldon, Lamport and Blakeman, 1856.

[43] Boston *Liberator*, December 2, 1859, citing the *Taunton* (Mass.) *Republican*.

MEDICAL ELECTRONICS—PROMISE AND CHALLENGE

VLADIMIR K. ZWORYKIN

Honorary Vice-President, Radio Corporation of America

(*Read November 10, 1960*)

In a day when science and technology have combined to revolutionize our environment and endow us with unprecedented material wealth, it takes a certain temerity to dwell at length upon one vital sector in which promise and fulfillment remain far apart.

The promise to which I refer is the experimental adaptation of electronic techniques to medicine. The fulfillment is the widespread application of these techniques to medical practice as a whole, on a scale sufficient to serve the needs of a swiftly growing national and world population. Constructing the bridge that will join these two extremes offers a challenge that we are only now beginning to appreciate.

In the past few years, the *potential* utility of electronic techniques in biological research and medical practice has come to be widely accepted among electronic and medical specialists. Their interest has resulted in the formation of joint organizations to explore and encourage the application of these techniques. Such organizations are at work today in this country, the United Kingdom, France, and Japan.

On the international level, the degree of interest in the problem was made evident last July when some eight hundred participants from twenty-four countries attended an International Conference on Medical Electronics, held in London under co-sponsorship of the International Federation of Medical Electronics and the British Institution of Electrical Engineers. Furthermore, the Senate Commitee on Reorganization and Government Operations is now engaged in a study of world-wide private and governmental activities in this same field.

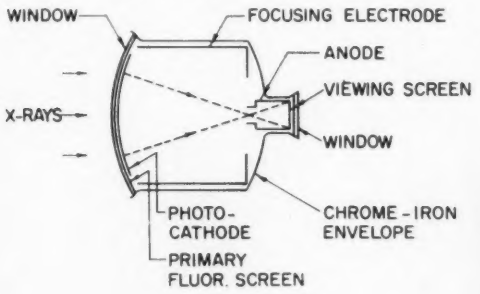
It stands to reason that this expanding interest in medical electronics is based on some solid accomplishments. Such accomplishments have, indeed, been realized. However, it is not my intention to dwell on them. Instead, I intend to draw on my own experience in a number of areas of medical electronics to point out some important problems which must yet be solved to realize the promise which electronics holds for medicine. We shall see readily that all that has been done so far pales into insignificance when measured against the work which is yet to be done, although the potentials for accomplishment are great.

Let us begin with an area in which the contributions of electronics to medicine are universally utilized, the area of observation. X-rays have long been indispensable for the study of structures within the body, particularly skeletal structures, calcified lesions, and organs or vessels which can be outlined by the ingestion or injection of contrast-enhancing media. A related technique uses as contrast media nuclear radioactive tracers which, after injection or absorption, are scanned by scintillation counters, another product of electronics, to localize neoplastic or other abnormalities.

More recently, electronics has coped with a related problem—minimizing the tissue damage incidental to the use of x-rays for diagnostic purposes, particularly in fluoroscopy or continuous x-ray observation. By providing image amplifiers of various forms, electronics permits a reduction in the x-ray exposure which is received by the patient in fluoroscopy by factors of the order of 100 or more, greatly reducing the risk incurred. One of the tools used for image amplification is the image converter tube, or image intensifier employing photoemission electron acceleration, and electron-optical size reduction to convert an image formed on an x-ray screen into a smaller image of much greater brilliance which may be viewed with a magnifier (fig. 1). A second is a television system with a high-sensitivity television camera focused on a conventional x-ray screen and reproduced by means of closed circuit television on a conventional television receiver; and, more recently, a panel x-ray image amplifier consisting essentially of a sandwich of photoconductive material sensitive to x-rays and of electroluminescent material, across which an alternating voltage is applied (fig. 2). X-rays render the photoconductive material conductive and enhance in this manner the electric field within the electroluminescent layer, which emits light in conse-

quence. The x-ray panel intensifier has the particular advantage that, with it, observation techniques are essentially the same as with a conventional screen; only the picture is much brighter. However, at this date it suffers from both an excessive time lag and objectionable picture granularity. Further work to remove these defects is needed before it can achieve maximum utility.

Parallel technique for observing internal structures, applicable in particular to the visualization of pathological changes in soft tissues which are not revealed by x-rays, is as yet in its infancy. This is ultrasonic ranging, adapting the experience of sonar and radar to medicine.



11 INCH X-RAY IMAGE INTENSIFIER
(Philips)

FIG. 1. Converter tube-type image intensifier.

Electronics has also given the life scientist his most powerful tool for examining structure down to the molecular level—the electron microscope (fig. 3), an outgrowth of the electron-optical studies basic to the modern cathode-ray tube and electronic television. Much of our knowledge of morphology, which had previously been derived from indirect experiments, has been put on a firm basis by the electron microscope.

Unfortunately, the intense electron bombardment and the evacuated environment required in the electron microscope prevent the observation of living matter with this instrument. Other factors have frequently led to the same condition in the study of structural detail in cells and microorganisms with the light microscope. To distinguish different chemical constituents in the cell it has been necessary to employ staining and fixing techniques which killed the cell and left the question open whether significant artifacts were introduced by these techniques. Without staining, most of the interesting structures were uniformly transparent. While interference microscopy revealed morphological detail in such structures, based on small variations in the refractive index of the constituents, it did not permit the simple correlation between chemical constitution and color which is obtained with staining.

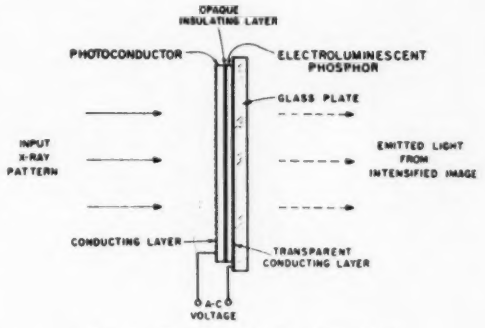


FIG. 2. Panel-type x-ray intensifier.

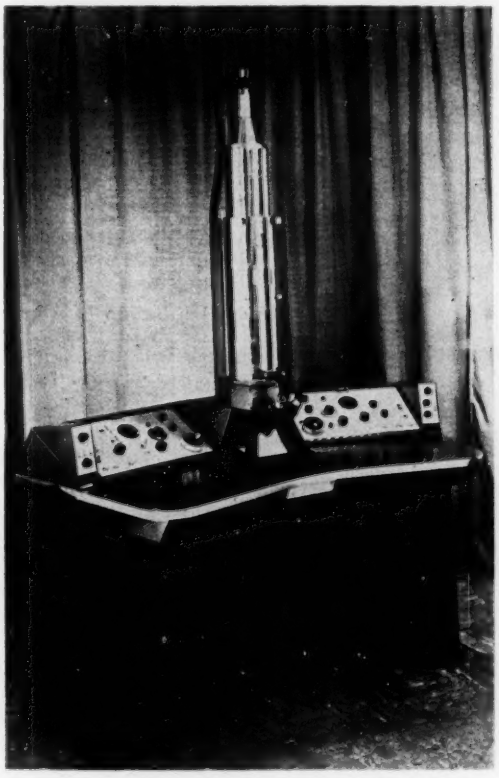


FIG. 3. Commercial type of electron microscope.

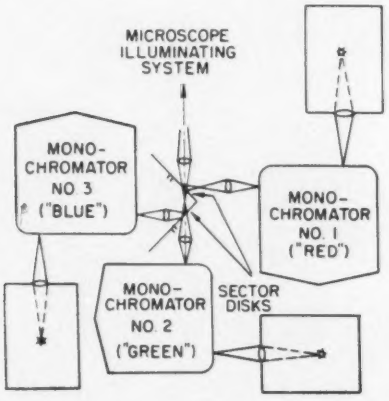
In our work with the ultraviolet color-translating television microscope at the Rockefeller Institute, we set ourselves the task of obtaining the benefits of staining—the identification of different chemical constituents by their color—without its drawbacks, namely the chemical treatment of the specimen resulting in its death and potential artifacts. It depended on the well-known fact that the very organic materials which appear perfectly transparent in the visible part of the spectrum exhibit highly distinctive absorptions in the ultraviolet. Unfortunately, our eyes are insensitive to the ultraviolet and hence cannot appreciate these differences—even if they could tolerate exposure to ultraviolet radiation. Photographic plates are sensitive to these radiations, but will only provide monochrome pictures of the specimen, albeit revealing much greater detail than pictures of the same specimen taken with visible radiations. However, as Brumberg pointed out in Russia some twenty years ago, if three photographs are taken of the specimen with ultraviolet radiations of different wave length and these are projected in superposition on a screen through red, green, and blue filters respectively, a composite image is obtained in which the color of different areas is indicative of their chemical constitution, as expressed in their differential absorption for the three ultraviolet wave lengths utilized.

The photographic color-translation technique, which was developed by Land and his co-workers and, at a later date, by Hovnanian and Holt, is limited in usefulness by the time delay between exposure and observation as well as by the relatively complex processing procedure. We realized, however, that this drawback could be readily overcome by the application of color television techniques. In the color translation microscope at the Rockefeller Institute,[1] the specimen is illuminated in turn by three pulsed arc lamps whose radiation is filtered by three separate monochromators (fig. 4). A rotating 45° mirror directs the radiation into the substage of a katadioptric ultraviolet microscope, which projects the image on to the target of a special ultraviolet-sensitive image orthicon. The amplified signal output of the image orthicon is applied in turn to the three guns of a color-television viewing tube through a gating circuit actuated by pulses derived from the rotating base of the 45° mirror. The same pulses time the discharges through the three arc lamps and synchronize the vertical deflection of the television system.

Thus, the instant the 45° mirror is in position to reflect radiation from a particular monochromator slit into the axis of the microscope, a light flash from the corresponding arc lamp projects an image on the image orthicon target. This light flash occurs in the fly-back period preceding the field period during which the gun for the primary color correlated with the monochromator wave length traces out a picture on the viewing-tube screen. Thus, in three successive field periods of 1/20 second a complete composite three-color picture of the specimen is formed on the viewing screen.

With this instrument we have been able to study not only the distribution of various materials, such as nucleic acid and hemoglobin, in stationary specimens such as the nucleated red cells of frog blood, but also protoplasmic streaming, the process of locomotion in amoebas, and chlorosis in plant cells. In essence, the process of observation is no more complex than for microscope observation with visible light. Engineered so as to demand a minimum of adjustment and maintenance, the ultraviolet color-translation television microscope could become an extremely effective instrument, not only for research, but also for diagnostic clinical use.

The ultraviolet color-translating television microscope can be regarded as a particular illustra-



COLOR TRANSLATING MICROSCOPE ILLUMINATOR EMPLOYING SECTOR DISKS

FIG. 4. Schematic drawing of illuminating system for color-translation microscope.

[1] Zworykin, V. K., and F. L. Hatke, Ultraviolet television color-translating microscope, *Science* **126** (3278): 805–810, Oct. 25, 1957.

tion of the ability of electronics to select out a piece of information of particular interest and to present it in a form which is most readily assimilated by the observer. In the example here considered the desired piece of information is the presence or absence of a particular chemical constituent, while the mode of presentation is to label it with a particular color. Electronics can, however, go much farther in selecting out a particular item of information. As an example, we may cite the Cytoanalyzer of Walter Tolles and his associates at the Airborne Instruments Laboratory. The objective of the Cytoanalyzer is to determine whether any cancer cells are present in a Papanicolaou smear on a microscope slide. Cancer cells differ from normal cells in the larger diameter and greater optical density of their nuclei. In the Cytoanalyzer a minute light spot sweeps out a scanning pattern on the slide. The transmitted light falls on a phototube, the output of which is applied to a series of electronic computing circuits. The primary purpose of these circuits is to measure the lengths of transit of the light beam across the several cell nuclei and the corresponding optical transmission. Signals proportional to the length of transit and to the optical transmission are then applied to the horizontal and vertical deflection respectively, of a cathode-ray tube, producing a luminous dot on the tube face, which is photographed by an open-shutter camera. The tube face is marked off to separate dots falling in the normal range of transit lengths and densities from those which may correspond to cancer cell nuclei. If a significant number of dots falls in the abnormal range, the smear is referred to a pathologist for individual scrutiny. In the converse case, the smear examination by the Cytoanalyzer can be regarded as negative. In this manner the Cytoanalyzer can materially reduce the work load which early cancer diagnosis by the Papanicolaou technique imposes on the pathologist.

Actually, the role of the computing networks of the Cytoanalyzer is considerably more complex than described above. It is not only necessary to measure the transit time pulses and signal amplitudes which correspond to cell nuclei, but to reject artifacts which may result from folded or overlapping cells as well as from various forms of debris which may be present on the slide. Otherwise, all slides would present so many suspicious dots on the display that the technique would be useless for discriminating between slides with and without cancer cells. To minimize the appearance of spurious dots, all pulse measurements are rejected for which the signal amplitudes before and after the transit do not fall within a range appropriate for the cell protoplasm. It is clear that this procedure requires a rather complex arrangement of gating and delay circuits.

The problem of gleaning the significant from the mass of information which impinges on us from all sides is, in fact, ever present. It has been solved for us in many everyday situations by built-in mechanisms which may be regarded as the products of evolution. Thus, relatively minute motions in an otherwise stationary field of view immediately call attention to themselves. Again, we may remain quite unaware of the content of a conversation going on nearby until some word of special significance to us, such as our name, is mentioned. This may shift our attention, at least temporarily, to a stimulus we had ignored until then.

Nature has not prepared us however, to deal in the same way with scientific data. Here we are in frequent need of data reduction or presentation devices which separate out the important elements of information. More commonly than not, these devices must be electronic to permit adequate speed in the processing of the data.

A good example is provided by the encephalogram. The usual encephalographic record is a trace of the potential variation between an electrode making contact with the scalp at a particular point and a "reference electrode" so placed that it is influenced only slightly by potential variations in the cortex or that between two different scalp electrodes. Commonly some six traces from differently placed electrodes or electrode pairs are recorded in parallel. At first glance, the potential variations resemble the random noise fluctuations observed in electrical circuits. However, closer inspection reveals the presence of characteristic periodicities in the range of 0.5 cycle to about 30 cycles per second. Since relationships have been established between the amplitudes and frequencies of these "rhythms" and external stimuli as well as certain pathological conditions, they have attained considerable importance both for diagnosis and for physiological research. It is thus of particular interest to select out these rhythms from the undifferentiated, random background. Several techniques have been developed for this purpose. One of these is the Fourier analysis of segments of the trace for fixed time intervals of e.g., 10 seconds. Another technique developed in particular by Norbert Wiener and his associates at the Massachusetts Institute of Technology, is corre-

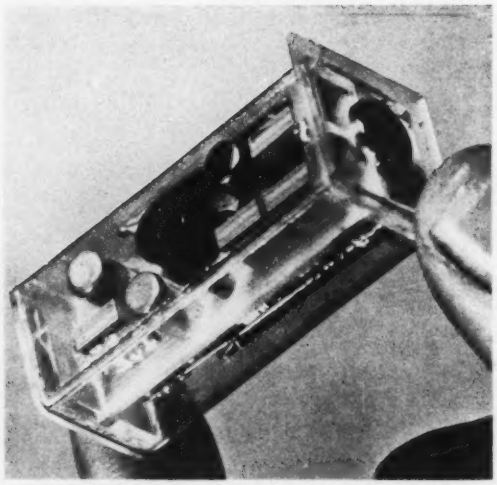


FIG. 5. Miniature amplifier for displaying skin potential.

lation analysis. The autocorrelogram of a trace is the time average of the product of the trace amplitude with the amplitude of the same trace delayed by a fixed interval, plotted as a function of the interval. For noise, the correlogram is a uniformly decreasing function; any periodicity in the trace is reflected in a periodicity superposed on the decline of the correlogram. Phase differences between periodicities in two different traces, having their origins in identical processes within the brain, show up readily as phase differences between the periodic variation of the cross-correlogram of the two traces and that of the autocorrelogram of one of the traces.[2] Still other approaches involve matching the wave shape of the periodic EEG pulses with rationally chosen function families such as the Gram-Charlier series.[3]

Whatever methods of analysis are employed, the statistical evaluation of the EEG records demands the processing of very large quantities of data in relatively short time intervals. The electronic devices best adapted for this purpose are generally analog computers. Thus, for the determination of the Fourier coefficients wave analyzers are suitable, while for correlation analysis magnetic drums provide a convenient storage means, since a displacement of a reading head can supply the desired variable time delay.

The various methods of studying the electroencephalographic record which have been described, all aim at utilizing the time variation of the observed skin potentials either for diagnosis or for gaining a better understanding of the functioning of the nervous system. Space variations of these potentials can be studied in this manner only very indirectly and with limited resolution, corresponding to the small number of electrodes. A proper understanding of the space *and* time variation of these potentials is obtained most readily with the aid of area displays, first introduced by Stanford Goldman and his associates at the Massachusetts Institute of Technology and applied in particular by John C. Lilly, at the University of Pennsylvania and the National Institutes of Health, to the study of brain potential waves elicited by external stimuli in laboratory animals. In Lilly's experiments a rectangular array of electrodes was placed in contact with the exposed cortex. The voltage of each electrode was amplified and controlled the brightness of an indicator tube on a panel which was geometrically similar to the electrode array. This panel was photographed by a high-speed motion picture camera. For study, the pictures were reprojected at e.g. $\frac{1}{4}$ of the original speed. It was found that various stimuli gave rise to characteristic patterns of potential waves sweeping across the surface of the cortex. A recent development in Russia is the "Encephaloscope" which uses time sharing to obtain a 50-channel display.

It is interesting to consider how the latest techniques of solid-state electronics might be applied to simplify Lilly's method of studying space and time variations of skin potentials by means of area displays, and thus to render it more generally available for research and diagnostic purposes. We can readily imagine how a single sandwich system, with the skin electrodes at the bottom, an electroluminescent layer at the top, and solid-state amplifier stages in the intermediate layers could provide a visual picture of the potential variations at the skin surfaces which could be either viewed directly or be photographed by a high-speed motion picture camera for subsequent reprojection. Such a device, if available, would without doubt, find wide acceptance for the study and diagnosis of heart and brain defects.

Some preliminary work in this direction has

[2] See e.g. Lee, Y. W., T. P. Cheatham, and J. B. Wiesner, Application of correlation analysis to the detection of periodic signals in noise, *Proc. Inst. Radio Engr.* **38**: 1165–1171, 1950, and Kaplan, L. M., The design of electronic correlating equipment to be used in medical research, *WESCON Record*, part 5: 206–215, 1958.

[3] Saltzberg, B., N. R. Burch, M. A. McLennan, and E. G. Correll, A new approach to signal analysis in EEG, *I.R.E. Trans. on Medical Electronics* **PGME-8**: 24–30, July, 1957.

been done in our laboratory with the aid of miniaturization techniques. It was found possible to construct a solid-state multi-stage amplifier about 0.5 cu. in. in volume, sufficiently sensitive to record the EKG potential as well as pulses due to muscular functions and convert them into light (fig. 5).

Figure 6 shows the comparison of heart cardiograms made by conventional EKG with one obtained by this miniature unit. By the combination of a hundred or more of such units into a matrix, as shown in Figure 7, it is possible to observe directly the movement of this potential. Such a device, if available, would without doubt find wide acceptance for the study and diagnosis of heart and brain defects.

The area display panel which I have described is only one example of the many innovations in the medical field which have become possible through the trend toward miniaturization in electronics. Another well-known example is the radio pill. This consists in effect of a radio transmitter sufficiently minute in size to be ingested and passed through the entire intestinal system without any adverse effect on normal body functions. On its trip, which may last from one to four days, the location of the pill may be determined at any time by fluoroscopy. The plastic pill, which is slightly over an inch in length and 4/10 inch in diameter, contains a transistor oscillator energized by a minute battery and frequency-modulated by the ambient pressure. Thus, an externally located frequency-modulated receiver can continuously record the pressure within the gastrointestinal system at the location of the pill. As compared with other pressure-measuring devices, the radio pill has the advantage of minimum discomfort to the patient and accessibility of all parts of the gastrointestinal tract for measurement.

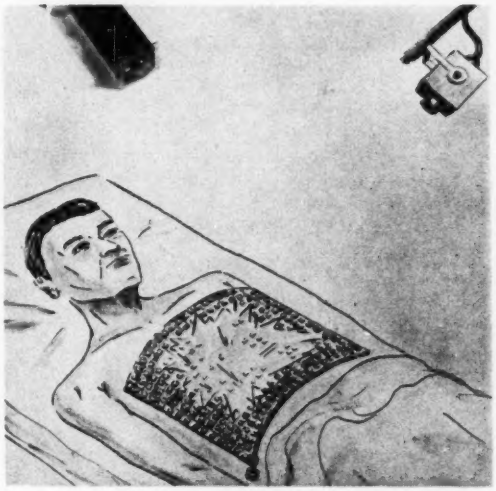


FIG. 7. Matrix of many individual potential probes.

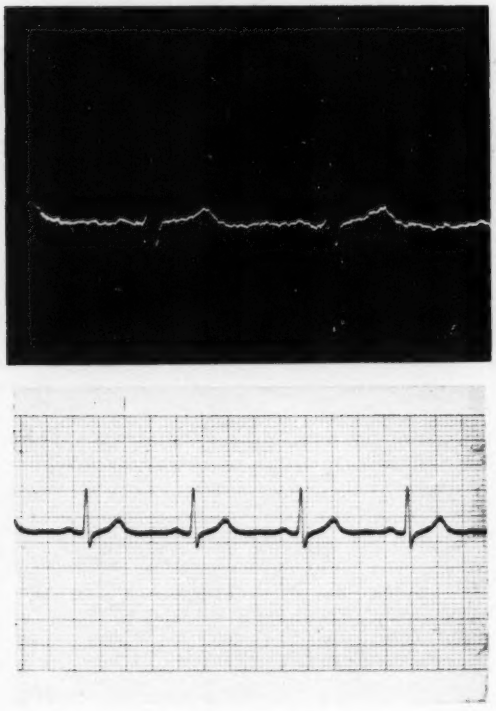


FIG. 6. Electrocardiogram from the amplifier probe of fig. 5 compared with a conventional one.

The pressure variation in the gastrointestinal tract is one physiologically important parameter for the functioning of the gastrointestinal system. Others are the temperature and acidity or hydrogen-ion concentration. Relatively minor modifications of the radio pill as described show promise of permitting the measurement of these parameters also.

The useful life, as well as the size of the radio pill described is defined by life and size of the battery. There are many applications which require the placement of measuring devices in the body permanently with the facility to interrogate it periodically. This has been accomplished with what we call the "passive pill." It consists of a tuned oscillating circuit excited from an outside transmitter (fig. 8).

In the radio pill, an electromagnetic field linkage

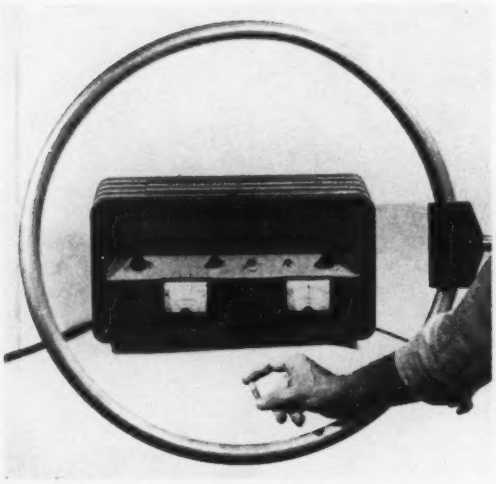


FIG. 8. Telemetering equipment used with the "Passive Pill."

between an electric circuit within the body and an external signal recorder overcomes the limitations which the attachment of wires imposes on the motion of a measuring device within the body. The same technique has proved of value for the insertion of artificial pacemakers in patients with cardiac deficiencies which render them chronically prone to heart insufficiency. The stimulator developed by Dr. Alexander Mauro of the Rockefeller Institute is enclosed in a plastic envelope. It is surgically inserted into the chest wall, with the metal terminals of the two output wires penetrating into the heart muscle. Excitation is supplied to it from an external source by electromagnetic induction. In this manner, the piercing of the skin by electrical leads, a potential source of irritation and infection, is avoided. The development of compact battery-operated transistorized pulse generators, furthermore, should restore normal mobility to the affected patient. It has been found possible to prolong the life of cardiac patients by several months through the use of such devices.

The whole problem of the replacement and supplementation of deficient organs by electronic devices is, of course, in its infancy. It is, however, quite clear that the development of a great variety of transducers and amplifiers of extraordinary compactness and minimal power requirements which characterizes our present era of solid-state electronics opens up possibilities in this field which were scarcely thinkable a decade ago. Close cooperation of physicians, physiologists, and electronic engineers in the exploration of the primary needs and methods of meeting them should yield a rich harvest.

There is another vital area of medicine in which the necessity of applying electronic techniques is becoming more and more obvious. This is the field of diagnosis. The mass of medical experience which has a bearing on the diagnosis of disease is accumulating in our hospitals and research institutions at a rate far too rapid for proper utilization by medical specialists, not to mention the general practitioner. It is far too extensive to be stored in the memory of the physician and no adequate method has been found so far to give him access to it. Some initial efforts, it is true, have been made and are being carried on to develop diagnostic aids to the physician in specific disease areas. One of these, supported by the National Institutes of Health, is conducted by a team of physicians and mathematical analysts at the Rockefeller Institute. It seeks to write an electronic computer program which will indicate, on the basis of recorded hospital experience, diagnoses of hematological disease which are consistent with any reported list of symptoms. A sample program, developed some time ago by Dr. Lipkin, Dr. Davis, and Dr. Engle in cooperation with the Medical Electronics Center of the Rockefeller Institute and the RCA Electronic Data Processing Division, demonstrated that this could, indeed, be done. When the symptoms of a typical hospital case, selected from a list of ninety-eight standard symptoms, were submitted to the computer, it printed out the name of that disease or those diseases out of a list of thirty which matched the reported symptoms; furthermore, whenever no positive diagnosis was supplied, those additional tests were cited which might lead to a definite decision.

In preparing the program great difficulty was experienced in assigning reasonable relative weights to the various symptoms. This can, in fact, only be done by subjecting a large mass of past experience to statistical analysis. Furthermore, to benefit by more recent work, the diagnostic computer should be capable of modifying the weights continuously as further knowledge is amassed. All this suggests that the diagnostic computer of the future should have characteristics which are basically different from those of computers now in use. Correspondingly, in attacking the difficult problem of developing a computer program for medical diagnosis, we must keep in

mind the new techniques which computer scientists are beginning to explore and limit our considerations in computing methods realized in present equipment. Advancement in the computer art is so rapid that computer ideas which are now in their initial stages of realization may be expected to be translated into hardware by the time a reasonable diagnostic program has been worked out.

The primary advantage of present-day computers is speed. In the performance of arithmetic operations they far excel the human brain which is limited by the slow rate of propagation and generation of nerve impulses. On the other hand, their intelligence is quite limited; they can only perform operations which are prescribed in great detail. Furthermore, their behavior is generally rigidly deterministic; thus, the failure of a single component, breaking the complex chain of logic in a large computer, renders it inoperative until the damage has been corrected. While the chance of such a failure is reduced by the intelligent selection of components and the provision of parallel facilities, it increases inevitably with the size and capacity of the computer.

All this suggests an entirely new approach in computer design, taking a cue from the pattern of the nervous system. Here, we have reason to believe, we are dealing with a system which is not rigidly, but statistically determined. The large number of participating units does not reduce, but increases, the reliability of the total system. Furtermore, it is a learning, or self-organizing system; its logic is continuously modified to harmonize with the information, in the form of external stimuli and sensations, presented to it.

Active research efforts are now being carried on to develop computing systems which possess these qualities of learning and self-organization. Their realization will be an important step toward the creation of medical information centers which will provide the physician with the essential information, based on the sum total of past medical experience, concerning diagnosis and therapy for any group of symptoms with which he may be confronted.

Long before this goal has been reached, there will be many other services which electronic computer and communication techniques of a more conventional nature can perform for medicine and public health. One of these is the establishment of standardized health record stores for the entire population, immediately accessible to the physician charged with the care of any patient. Even now, the necessary information for setting up such a store is largely available; schools, insurance companies, industrial enterprises, and the selective service system all give physical examinations, so that scarcely any member of the community is overlooked. However, at the present time, the results of these examinations are widely distributed and rarely available when needed to reconstruct the medical history of a patient. A physician, when consulted, must usually rely on whatever tests he can perform at the time and on whatever the patient happens to call to mind regarding his past difficulties. This is wasteful of the physician's time and greatly detracts from the confidence with which he can make a diagnosis and prescribe a remedy.

A more detailed investigation has shown that regional health-record stores for the entire population could be set up at costs and with space requirements which would not be excessive in view of the benefits which could be derived from such a system. Furthermore, requests for specific health records from physicians could be handled automatically with the aid of facsimile systems installed in the physician's office without undue time delay and without overtaxing space available in the radio spectrum for such communications. An incidental advantage of centralized health-record stores would be the possibility of rapidly checking general health trends, tracing the course of epidemics, and studying the effects of environmental factors on public health.

In summary, there is scarcely any field in medicine to which electronics could not make a vital contribution. Some of these are obvious and well on their way toward realization. Others, quite as important, are more remote and will become fully defined only after prolonged and intimate cooperation between biological scientists, physical scientists, and electronic engineers. I would like to stress that this cooperation is essential not only for realizing the potentialities of medical electronics, but, more importantly, for maintaining the health and physical vigor of the population at a level adequate for coping with the problems of an ever more complex world.

This provides adequate motivation for the best efforts of all of us who have devoted a major portion of our effort toward the advancement of medical electronics.

It may perhaps be hoped that the present emphasis on the application of electronic science to medical problems will be matched by a corresponding dedication in other branches of our knowledge.

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